

ANÆSTHESIA,
OR THE
EMPLOYMENT OF
CHLOROFORM AND ETHER
IN
SURGERY, MIDWIFERY, ETC.

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TO
CHARLES J. HAMBRO, ESQ.,
BRANDBURY HOUSE, WILLESDEN.

MY DEAR SIR,

I venture to inscribe the following pages to you, to mark my deep and sincere esteem for your character; and because I know no heart that will rejoice more than yours over any abolition of human suffering. Besides, I cannot forget that, last year, in my earlier researches into the practice of Anæsthesia, I was encouraged to perseverance by no one so much as by yourself.

And, with every kindest wish to you and yours, believe me,

My dear Mr. Hambro,

Your faithful Friend,

J. Y. SIMPSON.

52, QUEEN STREET, EDINBURGH,
30th September, 1848.

PUBLISHERS' NOTICE.

THE following work comprises the substance of several essays written at different times by Dr. SIMPSON, of Edinburgh, and published in the Medical Journals of that city, and of the verbal statements of his experience in the use of Anæsthetic Agents, made at the meetings of some of the Medical Societies of Edinburgh, and reported in their proceedings. These various papers are now for the first time collected into one volume, by the author. In presenting it to the public, the American publishers feel assured that they are giving the most complete history of Anæsthetic Agents, their discovery, history, mode of preparation, chemical properties, mode of administration, application and benefits resulting from their use, particularly in obstetrics and surgery, which has yet appeared. The author does not confine himself to his own extensive experience, but has collected with great care the results as shown in the practice of the different public hospitals of Great Britain, Ireland, and, as far as could be obtained, of Paris, thus adducing the important evidence of medical statistics to show that the rate of mortality is actually diminished under their use, in addition to the abrogation of pain which they procure. He has also obtained the experience of several eminent private practitioners in regard to their value. The various objections, religious and otherwise, which have been urged against them are met and answered. The letter to Dr. Meigs, in answer to his objections, was received from Dr. Simpson too late to take its proper place, after the letter to Dr. Smith, (Chapter IV. Part II.,) and now forms the concluding chapter of the work.

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PART I.

ANÆSTHESIA IN SURGERY.

"The multiplied experiments to prevent pain in surgical operations, which bear so delightful a testimony to the humanity of their authors, will certainly, in the course of time, be crowned with success."—*Marz' Akcios—Letter to Herman Boerhaave.*

CHAPTER I.

MERE OPINIONS AND PREJUDGMENTS NOT SUFFICIENT TO SETTLE THE QUESTION OF THE PROPRIETY OR IMPROPRIETY OF ANÆSTHETIC AGENTS: ILLUSTRATION FROM THE HISTORY OF VACCINATION.

DURING the latter half of the last century, 30,000 individuals were computed to die annually of small-pox in England.* From the official returns of the Registrar-General, it appears, that in England and Wales the number that perish annually of this same disease at the present time is reduced to less than 10,000.† In England alone, therefore, the absolute mortality

* Dr. Gregory observes, "The total deaths by small-pox throughout England were estimated at about 45,000 annually."—*Cyclopædia of Medicine*, vol. iv. p. 402. Dr. Haygarth calculated the annual number of deaths from small-pox to amount to 38,000 in 8,000,000 of inhabitants.—See the data of his computation in his "*Sketch of a Plan to exterminate Small-pox*," 1793, p. 144. In making the various computations regarding vaccination in the text, I have, in order to avoid the possibility of error, kept all the calculations considerably below the ascertained data.

† During the five years from 1838 to 1842 inclusive, there died, on an average, 8893 individuals yearly of small-pox. In 1842, only 2715 died.—See *Sixth Annual Report of the Registrar-General*, p. 514. Formerly, 1 in about every 250 of the general population died annually of small pox; now, only 1 in about every 1700. In England, the registration of every birth and every death is properly enforced by law. If the registration of the vaccination of each child were enforced as rigorously as the registration of its birth, much disease, and many thousand human lives would thus undoubtedly be saved annually in Great Britain. Surely it is a subject well worthy of the attention of a benevolent legislature. We see the good effects of such interference in other European states. For, whilst in England, (the native country of Jenner,) still 1 in every 1700 inhabitants dies annually of small-pox,—in Austria, 1 in 4800 dies of this disease; in France, 1 in 11,000; and in Sweden, only 1 in 27,000. On the great extent of the number of

from small-pox is less by twenty thousand a year than it was half a century ago. If a similar rate of reduction in the number of deaths from small-pox holds good—as we have every reason to believe is the case—in the other kingdoms of Europe, then, out of the 220 millions of people that inhabit this quarter of the globe, 400,000 or 500,000 fewer die now of small-pox, than, with a similar population, would have died from this malady fifty years ago. In other words, according to this rate of computation, there are now preserved from death by small-pox in England, during the currency of a single half century, a number of lives greater in amount than the whole existing population of Wales. There are preserved in Europe, during the same period, a number of lives greater in amount than the whole existing population of Great Britain.

For this mighty triumph of medicine over one of the most loathed and dreaded forms of human disease and death, science stands indebted to the inestimable discovery of Dr. Jenner;* and every medical man is ready to allow, at the present day, that his discovery is not less remarkable in consequence of its gigantic results and amazing success, than in consequence of the singular simplicity and safety of the means with which that success is obtained. For no one now dreams of ever expecting any deleterious or dangerous consequences to ensue from vaccination; and, indeed, the performance of it has been mainly or entirely conducted, in some districts, by non-professional individuals,—by the priest, as well as by the physician,—by the nurse, as well as by the surgeon.

Yet at the time of Dr. Jenner's first public announcement of vaccination in 1798, and for many years subsequently, the proposal of substituting vaccine for variolous inoculation was encountered by various members of the profession, with incredulity and ridicule, and direct and determined opposition. The measure by which he taught medical science to save annually from death, thousands of human lives in England, and hundreds of thousands throughout Europe, was, on its first introduction,

individuals in society who remain unvaccinated, see some excellent remarks by Dr. Stark, in the *Edinburgh Medical and Surgical Journal*, No. 161.

* In answer to those who have affected to doubt entirely the utility of physic and physicians, medical science may proudly point to the results of vaccination. During the long European wars connected with and following the French Revolution, it has been calculated that five or six millions of human lives were lost. In Europe, vaccination has already preserved from death a greater number of human beings than were sacrificed during the course of these wars. The lancet of Jenner has saved far more human lives than the sword of Napoleon destroyed. On these devastating European wars England lavished millions of money, and freely bestowed honours, peerages, and heavy annual pensions upon the soldiers who were most successful in fighting her battles and destroying their fellow-men; she grudgingly rewarded Jenner with twenty thousand pounds for saving twenty thousand of her subjects annually.

bitterly denounced and decried in different quarters,—its effects doubted,—and its own safety and propriety strongly and strenuously called in question.

Dr. Squirrell earnestly and publicly supplicated his Majesty George the Third to suppress “the destructive practice of vaccine inoculation throughout his dominions.”* “It ought,” observed Professor Monro of Edinburgh, “to be prohibited by Act of Parliament.”† “The College of Physicians have,” exclaimed Dr. Moseley, “a duty to perform, and I trust this business will not escape them.”‡ Others, despairing of interference on the part of the King, Parliament, or Colleges, appealed to the people themselves. “It would,” said Dr. Brown, “undoubtedly be downright madness to imagine *they* will condescend to encourage it.”§ The Anti-Vaccinarian Society called upon the public “to second their efforts in supporting the cause of humanity against cow-pox injuries,” and besought their aid to suppress “the cruel despotic tyranny of forcing cow-pox misery on the innocent babes of the poor—a gross violation of religion, morality, law, and humanity.”||

Frightful, and even fatal consequences were boldly averred to be the direct and immediate results of vaccination.

Deaths from cow-pox inoculation were published in the mortality bills of London.¶ “I have,” alleged Dr. Moseley, physician to the Chelsea Hospital, “seen children die of the cow-pox without losing the sense of torment even in the article of death.”** Dr. Rowley, physician to the St. Marylebone Infirmary, professed to publish true accounts of fifty-nine deaths from “cruel vaccination;” and added, that “when humanity reflects” on these and (to use his own words) “a great heap of victims diseased for life, and likely to transmit to posterity, for ages, beastly chronic diseases,†† it is enough to freeze the soul with horror.” And “it is,” he exclaims, “the duty of honourable men in the medical profession to alarm mankind of the impending danger of vaccination; to warn society of the multifarious evils that await them in the form of this mild catholicon, of a sweetened potion that carries fatal poison in

* Observations on the pernicious consequence of Cow-pox Inoculation. 2d ed. London, 1806. P. vi.

† Edinburgh Medical and Surgical Journal, vol. xv. p. 64.

‡ A Treatise on the Lues Bovilla. 2d edition. London, 1805. P. xiv.

§ A Letter in Reply to the Surgeons of the Vaccine Institution. Edinburgh, 1809. P. 96.

|| See their Address of 1806 in Blair's Vaccine Contest, p. 56.

¶ Mr. Blair's Pamphlet. P. 95.

** Treatise, p. 95.

†† Cow-pock Inoculation; with the Modes of treating the Beastly new Diseases produced by Cow-pock. 2d edition, 1805. P. 128.

all its destructive particles.”* He elsewhere eloquently declaims against “affectionate parents being robbed of their serenity, and the minds of tender mothers being wrung with eternal suspense,” “whilst a few projectors or visionists are pursuing their deleterious projects on human victims,” and perpetrating, a “dangerous innovation which so many fatal facts illustrate.”†

Mr. Lipscomb urgently maintained, in an essay on small-pox inoculation published in 1805, that cow-pox, the “new scourge industriously dispersed to afflict the children of men,” is “sometimes fatal of itself, and that the diseases introduced or brought into action by it may be also sometimes fatal, and can never be completely guarded against.”‡ One author had seen “numerous instances” of vaccination producing eruptions, remaining, “for months and even years afterwards, undermining the constitution, and very frequently terminating in phagedenic or corroding ulcers.” And he had likewise witnessed coughs, dyspnœa, hectic marasmus, tedious and difficult to eradicate, &c., result from cow-pox. “Shocking reflection (he adds) to a humane mind, that a poison should thus be introduced into the human constitution without the plea of necessity, or the support of reason and experience.”§ “Several children,” observes Dr. Moseley, “have died from diseases brought on by the cow-pox where no ulcerations had appeared, and others have lost their nails and ends of their fingers, several months after the inoculation.”|| “My accounts from the country are (he continues) full of dismal histories of ulcerated arms and mortifications.”¶ “Blindness (it was averred), lameness and deformity had been the result of employing the vaccine in innumerable instances, and its fatal venom had removed many an infant untimely from the world.”**

Nay, it was strenuously maintained and believed, that not only were various old maladies, peculiar to man, thus excited into action by the “cow-pox poison,” but that different new diseases peculiar to the cow were sometimes communicated to the human constitution by vaccination. “Various beastly diseases,” writes Dr. Rowley, “common to cattle, have appeared among the human species since the introduction of cow-pox, cow-pox mange, cow-pox abscess, cow-pox ulcer,

* Cow-pock Inoculation; with the Modes of treating the Beastly new Diseases produced by the Cow-pock. 2d edition, 1805. P. 14.

† Ibid. p. 128.

‡ Inoculation for the Small-pox vindicated, &c., 1805. P. 40.

§ Observations addressed to the Public on the Cow-pox, pointing out the dreadful consequences of this new Disease, so recently and rashly introduced into the Human Constitution. By R. Squirrell, M.D., 1805. Pp. 16, 17.

|| Treatise on the Lucæ Bovilla, p. 118.

¶ Ibid. p. 92.

** Moore's History of Vaccination, p. 39.

cow-pox gangrene, cow-pox mortification, and enormous hideous swellings of the face, resembling the countenance of an ox, with the eyes distorted, and eyelids forced out of their true situation; diseased joints, &c."*

This was published in 1806, eight years after Dr. Jenner's first essay on vaccination appeared. During the year subsequent to the first public announcement of his discovery, Dr. Moseley suggested the possibility of the "bestial humour" of cow-pox producing "a brutal fever exciting incongruous impressions on the brain;" and "who knows (says he) but that the human character may undergo strange mutations from quadrupedan sympathy, and that some modern Pasiphææ may rival the fables of old?" Some, after vaccination, were actually supposed to "cough like cows," and "bellow like bulls."† And one anti-vaccinist ingeniously suggested that if cow-pox were known to have existed in a family, this fact might debar the members of it from the chances of matrimony. For‡ "it would (he remarks) be no letter of recommendation, and it would be cruel for the world to know, who had laboured under the cow-pox mange, evil, ulcer, or any other beastly disease; it might infallibly injure their fortune in life, particularly in matrimonial alliances. Who would marry into any family, at the risk of their offspring having filthy beastly diseases?"

Nor were theological reasons, of course, wanting for calling in question the orthodoxy of vaccination, as of other new discoveries and practices.§ "Small-pox (argues Dr. Rowley) is

* Cow-pox inoculation. p. 105. See prefixed to the work the coloured portrait "of a cow-poxed, ox-faced boy," with two serofulous abscesses, which were at one time alleged to indicate sprouting horns!—"This boy (observed Dr. Rowley, in a clinical lecture on the case) is gradually losing the human lineaments, and his countenance is transmuting into the visage of a cow." (*Moore's History*, p. 46.) He further wrote—"A great number of new complaints, the diseases of beasts, filthy in their very nature and appearance, in the face, eyes, ears, with blindness and deafness, spreading their baneful influence over the whole body, have been not unfrequently the consequence evidently of cow-pox inoculation; either originating from the grease in horses, or the natural diseases of cows." P. 12.

† Mr. Ring, in his treatise on cow-pox, mentions "a lady who complained that, since her daughter was inoculated, she coughs like a cow, and has grown hairy all over her body; and Mr. Blair was told, on a late excursion into the country, that the inoculation of the cow-pox was discontinued there, because those who had been inoculated in that manner bellowed like bulls!"—*Blair's Vaccine Contest*, p. 69.

‡ Introduction to Rowley's pamphlet, p. vii.

§ As for example, small-pox inoculation: see a "Sermon against the dangerous and sinful practice of Inoculation, preached at St. Andrew's, Holborn, in 1722, by Edmund Massey, M.A. He urges various theological arguments against the 'diabolical operation' of inoculation, and at last maintains that, even if it were medically successful, it was not to be courted, for he believes if mankind should thus 'happen to become more healthy, it is a great chance but they would be less righteous.'" P. 26. In his admirable "Account of the Inoculation of Small-Pox in Scotland (1765)," Dr. Monro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a heinous crime." P. 5. "Clergymen (observes Dr. Baron, in his

a visitation from God, and originates in man; but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the Gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"* "The projects of these vaccinators seem (it was affirmed) to bid bold defiance to heaven itself, even to the will of God."† "Providence (reasoned another author) never intended that the vaccine disease should affect the human race, else why had it not, before this time, visited the inhabitants of the globe. The law of God (he continues) prohibits the practice; the law of man and the law of nature loudly exclaim against it."‡

In short, vaccination was opposed and denounced on a variety of grounds. It was alleged to be occasionally fatal in its consequences; to be liable to excite various diseased actions and predispositions; to produce diseases new to the human constitution: to "be impious, unthinking, profane and irrational;" to be an innovation, neither "established on the basis of reason, nor supported by the foundation of truth."§ "The vaccine," exclaimed one enemy to cow-pox inoculation, "was the damnedest thing ever proposed; he wished the inventors were all hanged, and he would give his vote for its being done."|| And strong pictures were hung up to the public eye of the miseries it would infallibly lead to, in case of the recurrence of epidemic small-pox. "In many families,"¶ writes an author whom I have already quoted, "there will be none to attend the sick; nurses will quit their patients for their own safety; and servants fly from their masters' houses to shun the pestilence. Then we shall experience an horrid scene of public and private calamity—brought on by a medical experiment; embraced without due consideration; extended by a rash transgression over the bounds of reason; and, after the

Life of Jenner, vol. i. p. 231) preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things (he adds) would scarcely obtain credence were it not that similar arguments and assertions have been employed against vaccination itself."

* Rowley's pamphlet, p. 9.

† Vaccine Contest, p. 84.

‡ Preface to the second edition of Dr. Squirrell's Observations, p. iv.

§ Blair's Essay, p. 83; and Lipscomb's Pamphlet, p. 28.

|| Moore's Reply to the Anti-Vaccinists, 1806. P. 14.

¶ Preface to "Treatise on Lues Bovilla," p. 23.

fullest conviction of its inutility, obstinately continued, by the most degrading relapse of philosophy that ever disgraced a civilized world."

Such were the chief forms of opposition and argument that were stoutly and vigorously urged against vaccination during the earlier years of its progress. They are the same by which many of the happiest and greatest improvements in our profession have each in turn been assailed at their first promulgation. From time to time in the march of medicine and other allied sciences, some earnest and expanded mind conceives and elaborates a great and novel thought, destined in its practical application to ameliorate the condition and promote the happiness of mankind. But hitherto almost as often as the human intellect has been thus permitted to obtain a new light, or strike out a new discovery, human prejudices and passions have instantly sprung up to deny its truth, or doubt its utility, and thus its first advances are never welcomed as the approach of a friend to humanity and science, but contested and battled as if it were the attack of an enemy.* Practical medicine, in its past career, is full of instances illustrative of this remark. Witness the history of the immense and now almost forgotten difficulties accompanying the first introduction of mercury, antimony, and cinchona-bark, into medical practice; or the stern obstinacy with which the ligature of arteries after amputation was long, long rejected, and cauteries and caustics pre-

* This remark holds true, for instance, with regard to small-pox inoculation, &c. &c. Lord Wharncliffe, in his edition of the letters and works of Lady Mary Wortley Montagu, after giving the history of her Ladyship's introducing the practice of small-pox inoculation into England from the East, observes:—"What an arduous, what a fearful, and, we may add, what a thankless enterprise it was, nobody is now in the least aware. Those who have heard her applauded for it ever since they were born, may naturally conclude, that when once the experiment had been made and had proved successful, she could have nothing to do but to sit down triumphant, and receive the thanks and blessings of her countrymen. But it was far otherwise. Lady Mary protested that in the four or five years immediately succeeding her arrival at home, she seldom passed a day without repenting of her patriotic undertaking; and she vowed that she never would have attempted it if she had foreseen the vexation, the persecution, and even the obloquy it brought upon her. The clamours raised against the practice, and of course against her, were beyond belief. The faculty all rose in arms to a man, foretelling failure and the most disastrous consequences; the clergy descended from their pulpits on the impiety of thus seeking to take events out of the hand of Providence; the common people were taught to hoot at her as an unnatural mother who had risked the lives of her own children.

"We now read in grave medical biography, that the discovery was instantly hailed, and the method adopted, by the principal members of that profession. Very likely they left this recorded; for whenever an invention or a project—and the same may be said of persons—has made its way so well by itself as to establish a certain reputation, most people are sure to find out that they always patronised it from the beginning; and a happy gift of forgetfulness enables many to believe their own assertion." *Letters and Works of Lady Mary Wortley Montagu.* Edited by her great grandson Lord Wharncliffe. Vol. i. p. 55.

ferred; or the professional and religious prejudices which the propriety of saving human life by inducing premature labour has encountered up to within the last few years. Further, every proposed improvement seems to be met with the same invariable array of objections and arguments. The discovery may be new, but the grounds of opposition to it are not new,—they are merely the old forms of doubt, and difficulty, and prejudice, used on former occasions, recalled and reproduced anew. Thus not only in their leading principles and spirit, but in most even of their minute details, identically the same arguments that forty or fifty years ago were urged against the propriety and safety of vaccination, or a hundred years ago against small-pox inoculation,* have, within the last few months, been again invoked and used against the employment of etherization. Time has amply proved how futile and inapplicable these arguments were as directed against vaccination. In truth, those forms of reasoning and opposition against the employment of cow-pox that, some forty or fifty years ago, appeared to many members of the profession to be perfectly conclusive and insuperable, now read and appear to us at the present day as in the highest degree illogical and absurd. History has been compared to a mirror in which we may study the faults of our predecessors, with the view of avoiding the same errors ourselves. The history of cow-pox is certainly calculated to teach us this one lesson, that in relation to the truth of any novel doctrine or practice, such as vaccination or etherization, adverse opinions and prejudgments are—however strongly entertained, or however strongly expressed—not in themselves adequate, as some, at the present time, would seem to believe, to decide the whole matter in dispute, either in one direction or another.† And the moral is obvious,—that while

* "The very same objections (writes Mr. Moore in 1805), accompanied with the same species of proof, were adduced against it (small-pox inoculation), as are now brought against vaccination."—See his reply to the Anti-Vaccinists, p. 70.

† I have been told, that any comparison between the progress of vaccination and etherization cannot be true in one respect, that vaccination was at once and generally received. The quotations in the text show the contrary; and many of the strongest adverse opinions which I have quoted were published in 1805–6, seven or eight years after Dr. Jenner published his first admirable essay on the subject in 1798. After Dr. Jenner published this essay, he went to London, and resided there for nearly three months; but during this time, "with all his efforts and those of his friends, he was unable in the metropolis to procure one person on whom he could exhibit the vaccine disease. Not one individual would submit to it. After Jenner left London, Mr. Cline made the first experiment tried in London with cow-pox, by inoculating it as a counter-irritant on the hip of a patient affected with morbus coxarius."—(*Baron's Life of Jenner*, vol. i. p. 150.) Jenner first tried artificial vaccination in May 1796. In March 1800, Mr. (afterwards Sir Matthew) Tierney wrote him from Edinburgh, where he was then a student, stating that "Dr. Gregory, the professor of phisic, knew very little about it, and, of course, did not encourage it. Mr. Anderson, a surgeon at Leith, is the

minds anxious to promote new and probable inquiries should not be intimidated and deterred from their pursuit by such pre-judgments on the part of others, those who are, on the contrary, anxious to suppress them, should not venture to base their opposition upon mere impressions and mere opinions only. The ultimate decision upon such investigations ever comes to be founded, not upon preconceived beliefs or hasty deductions, but upon the careful examination and evidence of a sufficient body of accurate and well-ascertained facts. During the last six months, etherization has been used to a considerable extent in British surgery; and at the present time, we are perhaps in a condition to turn and look back upon this past experience with it, in order to endeavour to form, from the existing facts and cases, a proper judgment upon its merits or demerits, and especially in order, if possible, to obtain some satisfactory light upon that all-important question in relation to its employment, viz., whether its adoption increases or diminishes the usual mortality consequent upon surgical operations. It was with this view that the present communication was undertaken.

But, in the first place, and before engaging with this more difficult part of the inquiry, let me briefly adduce the positive evidence which we possess of the effect of etherization in cancelling and abolishing the sufferings attendant upon surgical operations, and the best means of effecting this desirable object, the *a priori* objections to it, &c.

only person here who has tried it.”—(Ib. p. 376.) As late as 1805, the popular opinion in London was much shaken with regard to the propriety and safety of vaccination. “The influence,” says Mr. Blair, “of false rumours and distorted facts operated so strongly in the district of Bloomsbury and St. Giles, as to preclude even a single person from applying for vaccination at that dispensary.”—(*Pamphlet*, p. vi.) Dr. Moseley boasts (p. 13 of his *Treatise*, second edition), that at that date, 1805, the middle and inferior classes in London had “renounced the delusion,” and would not “expose their children to cow-pox.” Instances of more marked popular hostility against it were not wanting in the early history of cow-pox. Mr. Gooch states, that the first people he vaccinated in Hadleigh, Suffolk, “were absolutely pelted and drove into their houses if they appeared out.”—(*Baron's Life*, vol. i. p. 382.) In the town of St—n, Kincardineshire, a surgeon was lately used in a similar manner, for venturing to etherize a patient for extraction of a tooth. But still etherization has made more progress in months than vaccination effected in years; and already within a few short months a knowledge of it has spread over almost the civilized world. Within these few days, I received a letter of consultation from a lady, asking some directions for the use of etherization at her approaching confinement, in October next. The letter was from the Far West, and dated “Mount Morris, Illinois, United States.”

CHAPTER II.

PROOF OF ANÆSTHETIC AGENTS POSSESSING THE POWER OF ANNULLING
THE PAIN ATTENDANT UPON SURGICAL OPERATIONS.

ABUNDANT evidence to convince any unprejudiced mind upon this subject has already been accumulated in our periodical literature; and no one who has felt in his own person, or witnessed in others, the proper and perfect effects of etherization, can reasonably entertain any scepticism upon this point. In regard to it, I shall content myself with adducing the evidence which Dr. Forbes some time ago offered as the result of his inquiries on the subject, in the London hospitals. "For the purpose," he says, "of obtaining information on all the points of this most interesting subject, we personally questioned all the patients in the London hospitals, who, at the period of our visits, still remained in the wards after the ether operations. They were in all *fifty-four*, and the great majority had been the subjects of capital operations. They were unanimous in their expressions of delight and gratitude at having been relieved from their diseases without suffering. In listening to their reports, it was not always easy to remain unmoved under the influence of the conceptions thereby communicated, of the astonishing contrast between the actual physical condition of the mangled body in its apparent tortures on the operating table of a crowded theatre, and the really happy mental state of the patient at the time. The old story of the magician in the Arabian Tales seemed more than realized before us, the ether being like the tub of water, one moment's dip of the head into which produced a life-long vision in the dreamer's mind."* Every operator who has used anæsthetics can confirm these statements. Additional evidence is unnecessary.

* See Dr. Forbes' very able article on Etherization, in the British and Foreign Review for April, 1847, p. 554.

CHAPTER III.

CONDITIONS FOR INSURING SUCCESSFUL ANÆSTHESIA.

To produce, however, the full and perfect effects of etherization, it is necessary to conduct the process in conformity with certain conditions. These conditions it is not the object of the present communication to consider. But I will take the liberty of mentioning two or three leading points, the importance of which, in relation to the attainment of complete success, has become strongly impressed upon my own mind by a somewhat extensive experience in etherization during the last few months.

First, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherization, and during his recovery from it.* All talking and all questioning should be strictly prohibited. In this way any tendency to excitement is eschewed, and the proper effect of the ether inhalation more speedily and certainly induced. And, *secondly*, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapour as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect. Latterly, I have found that

* The area of an hospital operation theatre is hence, perhaps, not the most favourable place for securing all the advantages of etherization, or rather for shunning all its disadvantages. Lately, in a case in which Professor Miller performed partial amputation of the foot, in the Royal Infirmary, I etherized the boy who was the subject of it, in his bed in the wards. He was carried in this state up stairs to the operating theatre—the amputation performed—and the patient brought back again to his bed before he was allowed to awake. He was thus, at one and at the same time, entirely spared the moral shock and pain of being transported and carried in before a formidable collection of surgeons and students, and saved from the physical sufferings attendant upon the amputation itself; for he was perfectly unconscious of aught that had occurred, and, when he awoke, he was not aware that he had been operated upon, or had even left his bed. While being carried from the ward to the operating room, the sponge with which he was etherized was kept fixed over his face with a couple of common elastic letter bands. In our surgical hospitals, if a ward immediately adjoining the operating theatre were set aside for operation cases, it would in this way facilitate the process of etherization, and insure more certain and perfect results from it.

for surgical purposes, and when it is not necessary to keep up the etherization above five or ten minutes, by far the best and most perfect inhaler is formed by a large sponge of the common hollow conical shape, perforated artificially with a pretty large aperture at the apex, and placed over the face like a mask, so as to include both the mouth and nose in its concave base. At first, it should be held at a little distance from the face, and afterwards gradually advanced to it, in order to avoid exhibiting the vapour in too powerful and irritating a form for the first few inspirations. Its interior should, immediately before using it, be fully and freely saturated with ether—a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotizing dose of the vapour.* Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule;—not the principle of etherization, but the mode of putting it in practice being altogether to blame.—But *thirdly*, whatever means or mode of etherization is adopted, the most important of the conditions required for procuring a satisfactory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife until the patient is under the full influence of the ether-vapour, and thoroughly and indubitably soporized by it.

In the operating theatres of the Paris hospitals, the most triumphant successes, in the original trials with ether, were obtained by M. Velpeau, who differed from his fellows in one all-important particular only, namely, in the forbearance with which he waited for the complete insensibility of his patients before venturing to take his operating knife into his hand. Few men have had more practice in etherizing than Dr. Snow of London, who has been in the habit, for some time past, of thus assisting with their operations some of the first surgeons in the metropolis. Speaking of the stage of etherization required, he draws a proper distinction between two degrees of this state, the first, and slighter, in which the patient moves, and winces, and seems to feel pain at the moment, but without afterwards remembering it; and the second and deeper state in which

* When a prolonged effect is required, as in midwifery cases, an instrument is necessary—were it for no other reason than the saving of ether, and the prevention of its diffusion through the apartment. Within the last few days I have seen a pamphlet dated Boston, May 30, 1847, in which it is stated that for three months previously, all apparatus had been laid aside, and the sponge alone used for etherization, by Dr. Morton of that city—the gentleman to whom, I believe, the profession and mankind are really and truly indebted for first reducing into practice the production of insensibility, by ether inhalation, with the object of annihilating pain in surgical operations.—See *Some Account of the Lethæon*, by Edward Warren, p. 87.

there is no evidence whatever of pain being felt, far less remembered. And he adds, "In full four-fifths of the cases in which he had administered the ether, there was not the least flinch or groan during the cutting of the surgeon's knife. He considered cases of this kind the only truly successful ones, and believed that with proper care every case might be of this nature. When the patient exhibited signs of pain, although he might have no knowledge or recollection of it afterwards, the ether was only partially successful."* As a proof that such deep states of etherization are not accompanied with danger, I may mention here, though in the way of anticipation, that out of 39 surgical operations, "nearly all serious ones," in which Dr. Snow has exhibited ether at St. George's hospital, 2 only of the patients died, namely, 2 on whom amputation of the thigh was performed, after they had been previously reduced to an "extremely weak and emaciated"† state. Now, 2 deaths in 39 hospital operations, or 1 in 18, would certainly be regarded as a very satisfactory and favourable result under almost any circumstances, and either with or without ether. Dr. Peacock, in his official reports of the Edinburgh Infirmary for 1842 and 1843, has published‡ two tables showing the results of the "principal operations" in that Institution, from amputation and lithotomy down to the operation for harelip. The tables include 150 cases in all; and 32 of the 150 patients operated upon died, or about 1 in every 5. Excluding 57 cases of "extirpation of tumours in various parts," 32 in the 93 individuals on whom other operations were performed, or nearly 1 in every 3, died.

To produce the complete anæsthetic and soporific effects of the chloroform some conditions are necessary to be attended to. Without attending to these conditions, you will have failures. 1. The chloroform vapour must always be exhibited as rapidly and in as full strength as possible, if you desire to have its first or exhilarating stage practically done away with, and excluded; and you effect this by giving the vapour so powerfully and speedily as to apathize the patient at once. If you act otherwise, and give it in small or slow doses, you excite and rouse the patient in the same way as if nitrous-oxide gas were exhibited. 2. In order that the patient be thus brought as speedily as possible under its full influence, the vapour should be allowed to pass into the air-tubes by both the

* Medical Gazette, February 26, 1847, and Braithwaite's Retrospect of Medicine, Vol. xv. 1847, p. 409.

† Lancet for May 29, 1847, p. 553.

‡ Statistical Tables of the Royal Infirmary of Edinburgh for 1842, p. xix.; and for 1843, p. xviii. In the reports of other years the operations are unfortunately not tabulated on the same plan, and do not show the mortality dependent upon them.

mouth and nostrils,—and hence all compression of the nostrils, &c., is to be avoided. 3. The vapour of chloroform is about four times heavier than atmospheric air. And hence, if the patient is placed on his back during its exhibition, it will, by its mere gravitation, force itself in larger quantities into the air passages than if he were erect or seated. As to the best instrument for exhibiting the chloroform with these indications, the simple handkerchief is far preferable to every means yet adopted. It is infinitely preferable to any instrument yet seen, some of which merely exhibit it by the mouth and not by the nostrils, in small and imperfect, instead of full and complete doses; and with instruments so constructed, there is no doubt whatever that failures and exciting effects would ever and anon occur. Besides, inhaling instruments frighten patients, whilst the handkerchief does not; and mental excitement of all kinds, from whispering and talking around the patient, is to be strictly avoided, if possible. As to the quantity required to be applied to the handkerchief, it has been stated, that the average dose of a fluid drachm was generally sufficient to affect an adult; but I have latterly seldom measured the quantity used. We must judge by its *effects*, more than its quantity. The operator gathering his handkerchief into a cup-like shape in his hand, should wet *freely* the bottom of the cup (so to speak), and if the patient is not affected in a minute or so, he should add a little more. It evaporates rapidly; and you must not wet your handkerchief, and then delay for a minute or more in applying it. It must be applied immediately. Not unfrequently, when the patient was just becoming insensible, he will withdraw his face, or forcibly push aside the handkerchief. If you *then* fail to reapply it to his face and keep it there, you will be liable to leave him merely excited. But probably two or three inhalations more will *now* render him quite insensible. The simplest test of its full and perfect effect is some noise or stertor in the respiration. Cease it as soon as this is fully set in. But reapply it, of course, from time to time, if it is wished to keep up its effects.

Dr. Bennett, has spoken of the stertor or some other symptom being “serious.” Now, this and other terms are, it is believed, calculated to excite unnecessary fear. “Serious” was a relative and conventional term, constantly liable to be altered by increased knowledge and experience. Twenty years ago, travelling at the rate of thirty miles an hour would have been reckoned a very serious matter. Now-a-days every one knew it was not so. The tyro looks at first upon the symptoms of an aggravated attack of hysteria as very serious. The physician of more experience knows they are not so. The

stertorous breathing, the spasms, and almost convulsive symptoms, &c., sometimes produced by chloroform, may appear serious to those who have had little experience in the use of this agent. But every one who has seen much of its effects, knows that there is no danger following, but no inconvenience even left by such a show of serious symptoms.

The strength and purity of the chloroform employed are essential elements of success. Professor Gregory has examined about a dozen specimens which he had procured from various shops, here and in Glasgow. Several of them were by no means of the standard strength. A medical friend two days ago, asked me if I ever had failed obtaining the effects of chloroform upon any person. I replied, never in any one case. My friend added that he had, the night previously, been unable to influence a parturient patient, though he had given her an ounce. On inquiry, I found he had used chloroform from a shop where, according to Dr. Gregory's researches, it was sold under a specific gravity of 1·200, instead of being 1·480.

CHAPTER IV.

THE PREVENTION OF PAIN IN SURGICAL OPERATIONS ALLEGED TO BE UNNECESSARY AND IMPROPER.

THERE is one strange episode which has been frequently re-acted in the early history of several of those practical improvements, which we all now proudly refer to, and rejoice over, as among the greatest and most undoubted advances made in the past march of medical science. It is this. Some striking discovery happens to be made, or some great improvement suggested. Its worth and importance, betimes, are acknowledged by different members of the profession. Others, however, full of doubts and difficulties, conjure up to their own minds, and to the minds of their brethren, all the usual forms of objection to the new view that has been propounded; and the more ardent among these opponents always fix, and insist among other points, upon this special and singular ground of objection that—the disease or evil proposed to be remedied is comparatively and truly less obnoxious in its character than was in general previously imagined,—that its removal is, consequently, not a matter of much moment,—and that the new and artificial measure now suggested for that purpose is, probably, in its action and effects on mankind, really more dangerous and deleterious than the very state or malady which it was proposed to remove by it.

Take, for instance, as an example of the remark, the strong opposition offered first to variolous inoculation, and afterwards to vaccine inoculation.

It has been repeatedly calculated in regard to Europe, that before the introduction of variolous inoculation, small-pox regularly decimated the human race, one out of every ten deaths that occurred being a death from small-pox; and further, the disease was reputed fatal to at least one out of every 6 or 8 individuals attacked by it. Inoculation was introduced, and its effects were so marked that of those who submitted to the disease in this form, about one only in every 3 or 400 seems to have perished.* Vaccination was discovered, and this preven-

* In reference to the mortality of natural and of inoculated small-pox, Dr. Gre-

tive affection was found to prove rarely or never *per se* dangerous, or fatal to life. But science in substituting—first, inoculated for natural small-pox, and secondly, cow-pox for inoculated small-pox, was anxiously combated by the double argument, that the old evil was not so great an evil as it was usually represented to be, and that perhaps it was in reality safer and better than the new measure proposed as a substitute for it.

Thus, a hundred and twenty years ago, among the various pamphlets published with the view of contesting and opposing the propriety of the newly introduced Variolous Inoculation, Mr. Howgrave, whose Essay on the subject is one of the best of the period, maintains that “the small-pox in the natural way very rarely affects life where the habit of body and constitution are good;”* “that the hazard of inoculation is not only considerably less, but considerably greater than that of the natural small-pox;”† that this “new and strange method,”‡ “more frequently produces accidents than the natural way;”§ that it is “not only unsafe, but uncertain,”|| and that, “all persons who will suffer reason to determine their opinion, must be convinced that their (the inoculation) method has no degree of safety in it.”¶ Hence we can scarcely wonder when he “affirms that the best method is to trust Providence, and not allow mortal man to inflict diseases upon us at his pleasure.”** —Other cotemporary writers against small-pox inoculation, maintained upon the same grounds the same views of the impropriety of the practice.††

Again, forty or fifty years ago, in opposition to the proposal of Dr. Jenner to introduce vaccination, or, in other words, to substitute the inoculation of cow-pox for the inoculation of small-pox, the same objections to the practice were reproduced. Dr. Moseley stated,§§ for example, several years after vaccination was introduced, that “the mischiefs of the natural small-

gory, Physician to the Small pox Hospital of London, observes, “It is commonly stated that one-fourth of those who are attacked by small-pox in the natural way perish.”—*Cyclopædia of Medicine*, vol. iii. p. 742. And “The average number of deaths (from inoculated small-pox) at the Inoculation Hospital, was only three in a thousand (or about 1 in 330). In the wards appropriated to the casual diseases the deaths were, and continue to be, three in ten,” or more than 1 in 4.—*Ibid.* vol. iv. p. 749.

* Reasons against the Inoculation of the Small-pox. London, 1724, p. 67.

† *Ibid.* p. 72.

‡ P. 8.

§ P. 61.

|| P. 5.

¶ Reasons against the Inoculation of Small-pox, p. 28. ** *Ibid.* p. 63.

†† See for instance, the Rev. Mr. Delafaye's Sermon on “Inoculation, an Indefensible Practice,” p. 22, 23; or “A Letter in Reply,” by N. Bolaine, Surgeon, p. 17. Sparham's “Reasons against the Practice of Inoculating the Small-pox,” p. 27. Dr. Wagstaffe's Letter to Dr. Friend, showing the danger and uncertainty of Inoculating the Small-pox, pp. 48, 49, &c.

§§ Treatise on the Lucæ Bovilla, 2d edit. 1805, p. xv. xx. xxii. 110, &c.

pox, we all know, are great enough, but they may be prevented by inoculation ;" that, " instead of 1 in 300 dying of the small-pox from inoculation, there need not be one in 300,000 ;" " that the inoculated cow-pox is not a milder or safer disease than the inoculated small-pox ;" and that " the necessity of an immediate restoration of small-pox inoculation must strike every person interested in the welfare of society, and the happiness of his fellow-creatures." Cow-pox (averred Mr. Drew and Mr. Forrester) was a "*far more* severe disease than small-pox."*

" Out of many thousands, nay millions (so wrote Dr. Rowley in 1805),† it has been fully proved scarcely any one died from small-pox inoculation ;" " it was safe, mild, and certain ; therefore, cow-pox inoculation as a substitute was absolutely unnecessary ;" besides, the substitute itself " stands condemned by the experience of veterans in the profession ;" " disagreeable events (eruptions, abscesses, gangrenes, chronic diseases, &c.) have in great numbers followed, and can be traced to arise from cow-pox inoculation," while small-pox inoculation was free from them ; and " fifty-nine have died, and a great number of victims become diseased for life," in consequence of vaccination ; in fact " the senses," says Dr. Rowley, " are appalled, and the pen is tired of recording its dreadful disasters." (P. 61.)‡

But, in our own day, exactly the same line of argumentation that was thus zealously directed against the adoption of artificial variolation and vaccination, at *their* first introduction into practice, is now as zealously directed against the adoption of etherization. For precisely in the same way some minds, averse to the employment of ether inhalation, anxiously argue that there is not only no call for its employment in surgery, but that its employment, and the result which it produces—namely, immunity from the physical sufferings inflicted by the knife of the surgeon—is probably a direct and positive evil.

The common opinion of mankind and of the profession in

* See Lipscomb's Essay, p. 7, and Lipscomb's own similar opinion, p. 39.

† On Cow-pox Inoculation, pp. 4, 31, 100, 128, &c.

‡ See the works also of other anti-vaccinists for the same line of argument. Thus, observes Dr. Squirrell, the small-pox inoculation, " whatever impediment it might have met with at its introduction, owing, at that time, to a want of experience in the practitioners, is now a mild and harmless disease," and hence " there is no necessity to forsake its practice" for that of vaccination, with " all its difficulties, ambiguities, and malignant effects." " The cow-pox (he argues more at length) produces malignant effects, vitiates the blood, and other juices, and is tedious as well as difficult to cure; the small-pox inoculation produces no ill consequences whatever. The cow-pox produces very ill health in children; the small-pox inoculation improves the health and constitution, and carries off many complaints, which were very uncomfortable both to the parents and children. The cow-pox matter is taken from an animal diseased, and is of a specific scrofulous kind, as is proved by its effects ; the small-pox matter is taken from a healthy subject, and produces no disease whatever, but the one for which it was intended."—*Observations on the Cow pox*, pp. 24, 49, 55, &c.

regard to the pain attendant upon surgical operations, seems, till of late, to have been unanimous and unchallenged. The human agony and torture following the surgeon's knife, have hitherto been borne with and submitted to, merely because, while they seemed absolutely necessary for the preservation of health and life, they were considered at the same time absolutely unavoidable. It is true that differences in the mental and physical constitutions of different men, enable them to encounter the surgical mangling and mutilation of their limbs and bodies, with different degrees of equanimity and hardihood. And under special morbid states of the nervous system,—in the way both of great excitement and great overstraining and collapse,—flesh and bone have sometimes been extensively cut and divided without the common accompanying feelings, and common manifestations of acute suffering. But the every-day experience of mankind in every age, shows how greatly and truly the reverse of this is usually the fact. And all past human testimony on this point goes fully to corroborate the truth of the sentiment which England's great epic poet puts into the mouth of the wounded Nisroch, on the first occasion in which wounds were ever inflicted, and living beings first “knew pain:”—

“But Pain is perfect misery, the worst
Of evils, and excessive, overturns
All patience.”

Nor have we any proper test, either of the fortitude with which it will be borne, or of the amount of pain actually endured in individual cases. For whilst the degree of outward manifestation of suffering, shown by the patient on the rack of the operating table, affords no perfect evidence of his actual feelings (the greatest torture being incapable in some of contorting a muscle, or eliciting a groan, and the slightest scratch forcing screams and cries from others); neither, on the other hand, is the degree of equanimity and endurance with which the agony of a surgical operation is submitted to, any true and direct criterion of the natural moral strength and mental courage of the patient. Sometimes the sailor and soldier, who would not feel one moment's dread in facing, under the most desperate circumstances, the sword and fire of the enemy, will quail before the very thought of having his living flesh deliberately cut and mutilated by the cold steel of the surgeon.* And thus, the

* The following illustrative anecdote refers to Lord D——, assuredly one of the bravest admirals in the English service, and a man of all others whom his country has long looked upon as the very personification of valour and courage. The injury alluded to was received in cutting off the Spanish frigate A——, one of the most daring feats attempted in modern warfare. I extract the account from an article on Etherization in the “North British Review,” excellent

mere fear and horror of encountering the pain and agony of a surgical operation, will sometimes, by unnerving the strongest mind and boldest heart, bring on, as a consequence, such a depressed state of the system, as certainly by no means places the patient in a state favourable for securing a happy and successful result. "An extraordinary excitement of mind," observes Mr. Travers, "such as is produced by dread, or by the screwing up of the system for the endurance of painful operations, when it is already much depressed and enervated by continued suffering, or apprehension of it, sometimes proves suddenly fatal."*

In the greater operations of surgery, the employment of etherization is not only, thus far, a great blessing to the patient, but it is a great boon also to the practitioner. It is a benefit to the operator, as well as to those operated upon. For, whilst it relieves the one from all the feelings of agonizing pain, it relieves the other from the feelings of inflicting such pain upon a fellow-creature. Humanity, in the fullest sense of the term, is the great object of the healing art, and the aim alike of the surgeon as of the physician. Hitherto the professional duty of the surgeon has compelled him to inflict present suffering upon his patients, with a prospective view to their own ultimate benefit and advantage. And surely there is no one, however much inured to the sight and shriek of pain, who would not rejoice to be able to obtain these advantages for his patients, without compelling them to pass through so severe an ordeal as the tortures of the operating table. If, as some few operators themselves have indiscreetly boasted, their hearts have become so hardened by education and custom as not to be affected by the sufferings of those submitted to their knives, it is still pleasing and refreshing to know that this change in the human feelings, and this artificial violation of one of the first laws in human nature—namely, sympathy with the sufferings of others—is by no means necessary to make a man a perfect surgeon.

in matter, in manner, and in feeling, and written by one of the most distinguished surgical practitioners and authors of the present day. "We remember," says the reviewer, "the case of a gallant admiral—one of the bravest hearts that ever beat, in a service where men of every grade are, to a proverb, dauntless—who, in the opening of his distinguished career, had been engaged in cutting out an enemy's frigate. From the gun-boat, he climbed up the ship's steep side, and, foremost of his crew, had reached the bulwarks, when, receiving a stunning blow, he fell backwards into his boat again, striking his back violently on the tholpin. Many years afterwards, a tumour had grown on the injured part, and at length the admiral—gray, and bent in years, found it advisable that this growth should be removed. The man that never feared death in its most appalling form while in the discharge of duty now shrank from the surgeon's knife; the removal, contemplated with a feeling almost akin to fear, was long deferred; and at length, half stupified by opium though he was, a most unsteady patient did he prove during the operation."—*North British Review* for May, 1847, p. 169.

* Travers on Constitutional Irritation, Vol. I. p. 22.

Perhaps no surgeon ever operated, either more frequently or more successfully, than the celebrated Cheselden. In St. Thomas's Hospital alone, and exclusively of his practice elsewhere, he performed lithotomy upwards of two hundred times, and with results that have never yet been surpassed by any other operator. But he adds, in language bearing all the simple impressiveness of truth, "If I have any reputation in this way, I have earned it dearly; for no one ever endured more anxiety and sickness before an operation; yet, from the time I began to operate, all uneasiness ceased; and, if I have had better success than some others, I do not impute it to more knowledge, but to the happiness of a mind that was never ruffled or disconcerted, and a hand that never trembled during any operation."* It was under this great master that John Hunter received his first lessons in surgery; and the biographer of Hunter tells us that, (to quote his words,) "Cheselden's manners were exceedingly kind and gentle, and, notwithstanding the extensive practice he had enjoyed, he always, before an operation, *felt sick at the thoughts of the pain he was about to inflict*; though, during its performance, his coolness and presence of mind never forsook him. Such feelings," adds Mr. Otley, "are, in a less marked degree perhaps, far more commonly experienced than is generally supposed, by the very best surgeons, previously to undertaking operations of importance."† And, no doubt, it is the desire to diminish the actual amount of pain endured by patients, by curtailing the actual duration of it, that has led many of our best surgeons, with praiseworthy earnestness, to attempt to invent new and more rapid modes for performing particular operations,—a field in which no small degree of success has consequently been met with.

A new era, however, arrives in chirurgical science, and a measure is, at last, brought to light, through the influence of which surgeons may perform operations, and patients submit to them, even when of a prolonged nature, without the necessity of pain. It is found that the excruciating tortures, and writhings, and shrieks of patients on the operating table, may be saved; and yet the required operations be as well and perfectly executed as before. Scarcely, however, is this glad and glorious discovery announced and acted upon, than another new, and, if possible, still stranger discovery, is broached and anxiously promulgated; namely, that in cutting the living flesh of man, the surgeon's knife does not, after all, produce any very

* Cheselden's *Anatomy of the Human Body*, 5th edition, p. 333.

† Otley's *Life of John Hunter*, in Mr. Palmer's edition of his works, Vol. I., page 9.

remarkable, or very important amount of pain, and that immunity from this pain during operations would be, perhaps, an evil rather than a good to humanity,—a calamity rather than a blessing.

At a meeting of the South London Medical Society, held in April last, Dr. Gull read a paper on the injurious effects of ether inhalation, and ended his communication with queries as to the “*desirability* of removing pain,” &c.* Mr. Bransby Cooper, surgeon to Guy’s Hospital, afterwards affirmed it as his opinion, “that pain was a premonitory condition, no doubt, fitting parts, the subject of lesion, to reparatory action, and, therefore, he (Mr. Cooper) should feel averse to the prevention of it.”† “Pain,” argues Mr. Nunn, surgeon to the Colchester and Essex Hospital, in some observations against ether inhalation,—“pain [toothache?] is, doubtless, our great safeguard under ordinary circumstances; but for it we should be hourly falling into danger; and I am (he continues) inclined to believe that pain should be considered as a healthy indication, and as an essential concomitant with surgical operations, and that it is amply compensated by the effects it produces on the system, as the natural incentive to reparative action.”‡ Arguing in a similar but still more bitter strain against etherization, Dr. Pickford affirms, that “pain during operations is, in the majority of cases, even desirable; and its prevention or annihilation is, for the most part, hazardous to the patient.”§ Upon one of the first communications being given in to the French Academy of Sciences upon etherization, M. Magendie, the distinguished physiologist, maintained|| that “pain has always its usefulness;” he doubted if there was a true advantage “in suppressing pain, by rendering patients insensible, during an operation;” and argued, that “it was a trivial matter to suffer (*c’est peu de chose de souffrir*;) and a discovery whose object was to prevent pain was of a slight (*mediocre*) interest only.” When the effects of ether were discussed before the Medico-Chirurgical Society of Edinburgh, at one of their meetings in February, Professor Syme stated, that “he did not attach much importance to causing extinction of pain during operations;”¶ and more lately, he has published the opinion, that on “many” of those occasions in which he has seen the severest surgical operations performed, and “under the greatest liberties ever

* See Report of the Meeting in the London Medical Gazette for April 30, 1847, p. 777.

† Ibid.

‡ London Medical Gazette for March 5, p. 415.

§ On the Injurious Effects of the Inhalation of Ether, in the Edinburgh Medical and Surgical Journal for July, 1847, p. 258.

|| Gazette Medicale de Paris, 6th Feb. 1847, pp. 112, 113.

¶ Monthly Journal of Medical Science for April, p. 784.

taken with the human frame, such as the removal of large tumours, amputations, and lithotomy," the attendant sufferings of the patient were "nowise unbearable."*

It would be as idle as it would be useless to confute by mere argument the preceding allegations, in regard to the supposed necessity and advantages of pain in surgical operations. For, in fact, the whole question amounts to this:—Mankind are perfectly agreed, that the cutting and mutilation of the living human body *is* painful, however loudly surgeons may preach to the contrary; and medical men have hitherto assented, without one contradictory voice to the self-evident aphorism of Galen, that pain is useless to the pained (*dolor dolentibus inutilis est*). If we find then, as we do now, a few men entertaining and expressing opinions on these points so very different from the general ideas and general experience of mankind, these opinions can scarcely be looked upon as aught else than indications of a strange degree of eccentricity of thought upon one special subject. And if the same line of thought that they apply to pain were extended (as it should be if true in principle) to other diseases or effects of disease, the untenable and irrational character of it would lead to conclusions that might, perhaps, surprise and startle even those minds that at present employ it against etherization. For if physical pain and agony be a blessing and benefit, and not an evil and a calamity to be eschewed, then all other morbid symptoms and affections should equally, upon the very same grounds, be included under the same category,—their presence endured and courted, and their removal forbidden and avoided; diseases and death are parts of the great economy and general police of nature, and the labours of the physician and surgeon to counteract their agency, should be denounced and decried as unnecessary and improper.

Let us view the subject, however, calmly, and as a question in pathology. And that pain is, *per se*, and especially when in excess, a condition which is not only trying and difficult for any exertion of human fortitude to bear, but at the same time directly injurious and damaging in its action upon the constitution, and sometimes very fatal in its effects, is a practical truth that many of our best surgeons and soundest pathologists have long amply acknowledged and attested, and that, too,

* Ibid. for August, p. 75. Mr. Syme seems to believe that the very removal of a pain or "irritation of extraordinary intensity" may even produce death. In commenting upon the case of a patient, who had suffered "for nearly three years all the symptoms of stone in a degree of extraordinary severity," and who died four days after lithotomy was performed, Mr. Syme observes, "the fatal result in this case may, I think, with most probability be ascribed to the effect of suddenly removing a source of extreme irritation in a very irritable system." On dissection, the lungs were found "gorged with mucus," "white and firm."—*Edinburgh Med. and Surg. Journal*, vol. xxxv. p. 248.

without any prejudgment in regard to it, or, at all events, without any view to such an extraordinary proposition as the now alleged impolicy and impropriety of abolishing it.

Speaking of the pains of wounds, the great father of French surgery, Ambrose Paré, pithily tells us, that it "ought to be assuaged; because," adds he, "nothing so much dejects the powers of the patient (*prosterne et abatte les vertus du malade*)."* "Mere pain," observed the late distinguished Dr. Gooch, "can destroy life."† "Pain," according to Mr. Travers, "when amounting to a certain degree of intensity and duration, is of itself destructive."‡ "Pain in excess," he again remarks, "exhausts the principle of life; so that either its continuance without intermission, or the superaddition of the slightest shock subsequent to its endurance for a certain period, is fatal. In operations protracted by unforeseen difficulties, as in cases of lithotomy, in which the stone is of such magnitude as to require crushing, the patient has begun to die upon the table. The same happens in parturition, &c., protracted by mechanical impediment, &c."§ Again, "Bodily exhaustion from continued pain, spasm, and other causes, not unfrequently proves suddenly fatal. I have often observed a sudden deliquium at the conclusion of an operation for recent injury, arising evidently from the exhaustion of the nervous system in the effort to support 'it.'"|| "Great sensibility or excessive pain attendant on an injury (or operation) has," remarks Professor Burns, "two effects. First, it exhausts both the system and the part; and, secondly, it acts as an exciting cause of inflammation, a disease apt enough to take place from the injury itself."¶ "We have many facts," observes Professor Alison, "to prove that various violent and overpowering sensations, intense pain, &c., when acting in the utmost intensity, affect the circulating system just as a concussion does, and sometimes with fatal effect,—especially when they take place in a state of unusual

* Paré's Works, Johnston's Translation, p. 329.

† Mcrriman's Synopsis of Midwifery, 1826, p. 239.

‡ An Inquiry concerning that disturbed state of the Vital Functions, usually denominated Constitutional Irritation, 1826, p. 65.

§ Travers on Constitutional Irritation, vol. i. 76. The statistical data published by Dr. Collins in his excellent Report of the Dublin Lying-in Hospital, proves how true Mr. Travers's remark holds as a general principle with regard to the effect of pain in protracted parturition. In fact, the maternal mortality attendant upon parturition, regularly increases in a ratio progressive with the increased duration of the woman's sufferings. Thus (according to calculations which I have made from Dr. Collins's data) while in the women whose sufferings were terminated within two hours, only one in about 320 of the mothers died; where the labour varied in duration from two to six hours, one in 145 died: in those in whom it continued from seven to twelve hours, one in 80 died; when it endured from twelve to thirty-six hours, one in 23 died; and out of those whose sufferings were prolonged beyond thirty-six hours, one in every 6 perished.

|| Travers, p. 24.

¶ The Principles of Surgery, by John Burns, M.D. Vol. i. p. 502.

weakness or exhaustion.”* Dr. Ranking has lately published a striking instance of the fatally depressing effects of extreme pain. It occurred in a case in which a ligature was applied for the cure of an erectile tumour of the breast. “The patient,” he relates, “a healthy female, bore the initiatory steps of the operation without a murmur, without failure of pulse, and without change of countenance. The instant the ligature was tightened, which it was with the full force of two surgeons, she gave a yell of agony, the pulse became imperceptible, the countenance became ghastly pale, and in eighteen hours she was a corpse.”†

But it is, I believe, needless to accumulate superfluous proof on a point on which the observations and feelings of the profession and of mankind are sufficiently agreed; namely, that bodily pain, particularly such excess of pain as, with all its concomitant fears and sickening horrors, accompanies the larger operations in surgery, is, with very few, if indeed any exceptions, morally and physically a mighty and unqualified evil. And, surely, any means by which its abolition could possibly be accomplished, with perfect security and safety, deserves to be joyfully and gratefully welcomed by medical science, as one of the most inestimable boons which man could confer upon his suffering fellow-mortals. Few now deny, and, no doubt, ere long fewer and fewer will venture to doubt, that with a generality as certain and constant, or indeed more so, than we see exemplified in the action of most other therapeutic agents upon the human constitution, etherization *does* possess the desired and entire effect of abolishing and annulling the pain following the scalpel and saw of the surgeon. But, as I have already stated, a question of the most important moment remains behind. Many surgeons, who cannot gainsay the indisputable effects of ether-inhalation in cancelling the pains of surgical operations, still ardently maintain that this admitted amount of present good, can only be purchased and obtained by the patient, at the hazard or certainty of a greater and disproportionate amount of future evil. In other words, they allege and maintain that the condition of etherization is one which, from its marked powerful effects upon the economy, must produce, in some cases, immediately dangerous results, which must affect the system so as to impede and interfere with the condition of wounds, and the recovery of patients from them, and hence that

* Outlines of Pathology and Practice of Medicine, p. 13. See also p. 203 and 317, on peritonitis proving directly fatal, “by virtue, probably, of the intense and peculiar sensation (of pain) it excites, acting as a powerful sedative on the heart.” On pain as a cause of exhaustion of nervous power, see also Dr. Holland’s Medical Notes and Reflections, p. 618.

† Abstract of the Medical Sciences, by Dr. Ranking. Vol. v. 1847, p. 383.

it will render operations more perilous in their consequences, and more fatal in their ultimate results.

The correctness or incorrectness of this last feasible and assuredly most formidable objection to etherization in surgery, is a problem that no mere reasoning or mere opinion could ever certainly and satisfactorily solve. The evidence of simple prejudice and argument could never perfectly settle it, however plausible and ingenious the grounds of the prejudice and argument might be. It is one of those allegations, the accuracy or inaccuracy of which is a matter that can be fully and finally determined by one method only,—namely, by an appeal to the evidence of facts, and to the evidence of facts alone. For the purpose of assisting in the decision of this question, I have, through the great kindness of my professional brethren, collected the results of above three hundred amputations of the thigh, leg, arm, and fore-arm, performed within the last six months upon patients in an etherized state, in the civil hospitals of England, Scotland, Ireland, and France. The statistical analysis of these three hundred amputations with ether, and the comparison of *their* results with the results of various similar collections of the same amputations without ether, in the same and in other similar hospitals, will, I believe, enable us to arrive at some more definite ideas and deductions than we yet are in possession of, in regard to the debated question of the danger or safety of etherization in the operations of surgery. But let us first inquire if this statistical method is the proper method of investigating such a subject.

CHAPTER V.

VALUE AND NECESSITY OF THE NUMERICAL METHOD OF INVESTIGATION
AS APPLIED TO SURGERY.

"La possibilité de l'application de la statistique à la médecine, est une vérité tout aussi bien démontrée que la réalité de la circulation."—*Dezimeris, Dictionnaire de Médecine*, vol. xxviii, p. 550.

THE vast practical importance of the doctrine of statistics, and its power of elucidating, simplifying, and deciding many and various inquiries in surgical and medical science, is now becoming daily more and more acknowledged by the members of the profession. The doctrine itself has been long, not only acknowledged, but acted upon by governments and by the public at large. The political laws and expensive machinery pertaining to the registration of the deaths and diseases of the inhabitants of England, and of other kingdoms of Europe, are founded upon the soundness of the doctrine. In our numerous life assurances and annuity companies, millions of money are unhesitatingly staked upon the truth of it. And the principle upon which the usefulness and stability of the whole doctrine of medical statistics rests, is a very simple one. It amounts to this: Among facts, data, or unities of a variable chance—such as the probabilities of death within a given time, or the probabilities of attacks of particular diseases within a given time, or the probabilities of averting death in particular diseases by particular methods of treatment, or operation—there is ever a mighty uncertainty as to the results, if we consider only single cases, or a small and limited number of instances; but our results approach more and more to certainty, in proportion as we deduce these results from a greater and more extended number of instances,—from a larger and multiplied series of facts. There is always great uncertainty and instability in regard to the results of single or isolated cases; but a proper aggregation and conjunction of cases affords results which are comparatively certain and stable.

In the present investigation into the effects of etherization upon the mortality attendant on surgical operations, I have followed the statistical method of inquiry. But as the doctrine itself of statistics, as applied to such questions, is still, I fear, very imperfectly understood by the profession in this country,

I shall here take the liberty of premising a few observations upon this mode of inquiry.

I have already stated, that the great and leading principle upon which all statistical inquiry is grounded, consists in the fact, that in unities or entities of a doubtful chance, while the result, or event, in *individual* instances, is ever variable and uncertain, the result, or event, when calculated from, or upon *masses* of instances, becomes comparatively certain and invariable. I shall show the truth of this abstract remark, in the form of illustrations, of a few of the fundamental principles or propositions upon which the doctrine of medical statistics is founded, and this more especially with a view to the bearings and important advantages of the statistical or numerical method of inquiry as applied to questions in surgery.

FIRST PROPOSITION.—*The absolute number of deaths from all causes, in a given time, in a given population, is always nearly the same.* The probability of life or death to individuals within the limits of a given period, is proverbially uncertain. Nothing is more uncertain, for instance, than the number of individuals that will die in the currency of a single year in any particular family, street, or village in England and Wales. But nothing could be more certain than, *cæteris paribus*, the number that will die during the currency of a single year in the whole of England and Wales. Estimating, for instance, as we do in *all* modes of reasoning and philosophizing, from the experience of the past what will be the experience of the future under similar circumstances, we may state beforehand as certain, that in 1845 (the results of which have not yet been published) the total number of deaths in England and Wales amounted to about 350,000. For the returns of the Registrar-General for England and Wales have now been collected and published for seven years,—viz., from 1838 to 1844 inclusive, and the total numbers that died during each of these years were fixed and determinate, to the extent shown in the following table:—

No. I.—*Table of Absolute number of Deaths in England and Wales, and of the per-centage of deaths among the whole population during Seven successive years, from 1838 to 1844 inclusive.*

Year.	Absolute Number of Deaths.	Per-centage of Deaths among the Population.
1838	342,547	2·2 in 100
1839	338,979	2·1 in 100
1840	359,634	2·2 in 100
1841	343,847	2·1 in 100
1842	349,519	2·1 in 100
1843	346,446	2·1 in 100
1844	356,950	2·1 in 100

In reference to the preceding table, we must bear in recollection one point, that in this, as in other statistical inquiries, there is always a range of oscillation, and limits of possible error; but, as Gavarret has well demonstrated, the extent of these oscillations and limits of possible error are themselves easily ascertainable, and capable of being reduced to mathematical calculation and correction.

SECOND PROPOSITION.—*The absolute number of deaths from individual diseases and specific causes in a given time, in a given population, is always nearly the same.*—For if it be true, as shown under the first proposition, that the exact number dying annually in England is nearly the same, it is equally true and demonstrable that the particular causes or forms of disease producing these deaths recur in successive years in the same number and proportion. What is true regarding the whole, is true in regard to its parts. For the purpose of illustrating this secondary fact, I shall take from the Registrar-General's reports nine returns, three referring to medical, three to surgical, and three to obstetric affections and complications; and I shall add one pertaining to the department of medical jurisprudence. Each of them shows the comparative certainty of large numbers. For while, for example, no man could predict who or what number of a small community would die annually of croup, or tetanus, or ovarian dropsy, yet the absolute number dying each year of these and other affections throughout England, when calculated on a large scale, comes annually in all, except epidemic and zymotic diseases, to be nearly the same, as the following table sufficiently demonstrates. In fact, their numbers are, if possible, more determinate than the numbers of the total deaths; because, while the absolute mortality of a kingdom is liable to be varied by variations of a temporary and transient nature in the existing epidemic and endemic influences, &c., those individual diseases and causes of death, the etiology of which is more fixed, are more stable in their results.

No. II.—*Table of Absolute number of Deaths annually in England and Wales from twelve different Diseases or Causes of Death.*

Causes of Death.	No. Dying in 1838.	No. Dying in 1839.	No. Dying in 1840.	No. Dying in 1841.	No. Dying in 1842.
Croup,	4463	4192	4336	4177	4457
Jaundice,	841	800	875	864	952
Apoplexy,	5630	5293	5451	5581	5361
Hernia,	507	474	480	475	529
Tetanus,	129	122	142	118	118
Carbuncle,	35	38	33	28	40
Childbirth,	2811	2915	2989	3007	2687
Malformations,	166	214	211	206	217
Ovarian Dropsy,	45	34	43	44	52
Violent Deaths,	11,727	11,632	11,594	11,100	11,092

The regularity with which the same disease thus destroys, in successive years, nearly the same number of individuals, may appear remarkable to those who have not given attention to the study of medical statistics, and who have, consequently, not marked the fixed and determinate nature of the results which this means of investigation always elicits, when it is enabled to work upon a sufficiently large basis of facts, or a sufficiently large series of data. But this constancy appears, if possible, still more singular, when we return to such a subject as that included under the last column in the table, viz., "violent deaths." Under this head are included deaths by mechanical injuries, by chemical injuries, by asphyxia, &c., and, if the returns were more specific, it would no doubt be found that the number of violent deaths from *each* separate division of causes was annually nearly the same. Even causes originating in passions of the human mind, and leading to violent death by murder, are, *cæteris paribus*, repeated in nearly the same number in each successive year. The moral man is subject to laws as fixed as the physical man. Some years ago, Quetelet showed from the comparison of the annual number of deaths in Paris, and the annual number of crimes committed throughout France, that the statistics of human crime are as fixed as the statistics of human mortality; that each age paid as uniform and constant a tribute to the jail as it paid to the tomb; that the numbers of any specific crime in successive years was, like the numbers of deaths from any specific disease in successive years, always nearly the same; ay, that the very instruments by which the same crime (as murder) was perpe-

trated in different years, were always in nearly the same proportion.*

THIRD PROPOSITION.—*The absolute number of those that recover, should, cæteris paribus, be as fixed as the number of those that die from individual diseases in a given time, in a given population.*—The preceding table (No. II.) shows how many died of the several affections included under it, during a succession of years. If our statistics were more specific and detailed, we ought to be able to tell also how many recovered each year from attacks of each of these affections, as well as how many died from them; and if we could thus count the number of recoveries as well as the number of deaths, by striking the proportion between them, we would obtain the average mortality of each disease. The deaths, for instance, from croup, amount on an average to 4325 each year. But if at the same time we knew the total average number of cases of croup that occurred every year (say, for the sake of illustration, that they amounted to 13,000 in all), then the mortality of the disease would amount to nearly 1 in 3; or out of every three patients attacked with croup, two would recover and one die.

The Registrar's reports, however, do not furnish us with

* "If all human actions could be registered," says Quetelet, "it might be supposed that their numbers would vary from year to year as widely as human caprice. But this is not what we in reality observe, at least for that class of actions of which we have succeeded in obtaining a registry. I shall quote but a single example; but it merits the attention of all philosophic minds. In everything which relates to crimes, the same numbers are reproduced so constantly that it becomes impossible to misapprehend it—even in respect to those crimes which seem perfectly beyond human foresight, such as murders committed in general at the close of quarrels, arising without a motive, and under other circumstances to all appearance fortuitous or accidental. Nevertheless, experience proves that murders are committed annually, not only pretty nearly to the same extent, but even that the instruments employed are in the same proportions."—*Treatise on Man*, p. 6. The following table, abridged from Quetelet, may enforce still more the truth of his observations.

Table of the Annual Total Number of Murders, and Instruments of Murder, in France, collected from the Reports of Criminal Justice, from 1826 to 1831.

Modes of Murder.	In 1826.	In 1827.	In 1828.	In 1829.	In 1830.	In 1831.
Total Number of Murders, .	241	234	227	231	205	266
By Gun and Pistol,	56	64	60	61	57	88
By Knife,	39	40	34	46	44	34
By Stones,	20	20	21	21	11	9
By Kicks and Blows,	28	12	21	23	17	26
By Cudgel, Cane, &c., . . .	23	28	31	24	12	21
By Stabs, Cuts, and Bruises, .	35	40	42	45	46	49
By other means,	40	30	18	11	20	39

The difference in 1830 and 1831 from the preceding four years, were no doubt owing, in a great degree, to the Revolution of 1830, and its immediate effects.

returns of the recoveries as well as of the deaths in any special disease or complication, with one exception. Under the division of births he gives the number of children born each year, and under the term childbirth, in the division of deaths, he gives the number of mothers who perished each year under parturition or its consequences. Being thus furnished not only with the annual total number of deaths that took place from parturition, but also with the annual total number of cases of parturition that occurred, we have the data in this instance for calculating the proportion of recoveries to the proportion of deaths in childbirth. And the following table presents the results for the only four years of which the full data have been yet published.

No. III.—*Table of Proportion of Deaths in Childbed in England and Wales, from 1839 to 1842.*

Years.	No. of Children born.	No. of Mothers dying in childbed.	Proportion of Maternal deaths in childbed.
1839	492,574	2915	1 in 169
1840	502,303	2989	1 in 168
1841	512,158	3007	1 in 170
1842	517,739	2687	1 in 192*

FOURTH PROPOSITION.—*Statistics enable us to prove that the general mortality, the mortality in particular departments of practice, and the mortality from individual diseases, are capable of being altered by altering the attendant circumstances.*—Under the three preceding propositions, in showing the attendant results to remain from year to year the same, I have supposed the attendant circumstances to remain also the same. But if we change the conditions in which the community or the individuals composing it are placed, we change also the results. The great power which *art* possesses, is the power of altering these attendant conditions and circumstances by improved hygienic measures, improved medical practice, improved modes of operating, &c. And the effects of this alteration might be interminably questioned and doubted, provided

* In relation to the etiology and pathology of puerperal fever—the most common cause of death in childbed—it is not uninteresting to remark, that in 1842, when the deaths following parturition were so few, the number of deaths from erysipelas was also much diminished in number. This fact may, I think, be adduced as a reason additional to the many lately urged in proof of an identity in causation and character between erysipelas and puerperal fever.

we had not the power of proving it by simple statistical evidence. I shall adduce some examples.

Improve in regard to salubrity, &c., the circumstances in which a community is living, and you increase the value of life in that community, or diminish its mortality. Thus, in 1786, the yearly rate of mortality for the whole of England and Wales was 1 in 42; or, in other words, 1 in every 42 of the inhabitants died annually. In 1801, this mortality was found to be 1 in 47. In 1831, it had diminished to 1 in 58; showing a difference of 38 per cent. in the short period of half a century.*

Again, if we wished to prove that our practice, in relation to any particular department of the profession, was more successful now than formerly, or more successful under one mode of treatment than under another, our only certain form of evidence consists in a similar appeal to statistics. Mere assertions and opinions avail little in such a question. Figures and statistics can alone properly decide and determine it. And here, as elsewhere, they offer a kind of evidence, which is not less remarkable for its intelligibility and simplicity, than for its precision and certainty. I shall again take the case of childbirth as an example. The following table, calculated from the mortality bills of London, and given in a more extended form by Dr. Merriman,† demonstrates statistically—what could not be satisfactorily proved by any other form of evidence—that the practice of midwifery has become more and more improved in London, and inferentially in the whole kingdom, during the last two hundred years; the ratio of deaths in childbirth having gradually become less in number, and that to such a degree, that the proportion of parturient mothers lost, during the last years of the 17th century, was nearly double the number of those lost during the first years of the 19th century:—

No. IV.—*Table of average number of Mothers dying in Childbed in London from 1660 to 1820.*

Years.	Proportion of Mothers lost.
For 20 years ending in 1680 . . .	1 in every 44 delivered.
For 20 years ending in 1700 . . .	1 in " 56 "
For 20 years ending in 1720 . . .	1 in " 69 "
For 20 years ending in 1740 . . .	1 in " 71 "
For 20 years ending in 1760 . . .	1 in " 77 "
For 20 years ending in 1780 . . .	1 in " 82 "
For 20 years ending in 1800 . . .	1 in " 110 "
For 20 years ending in 1820 . . .	1 in " 107 "

* Dublin Review, vol. ii. p. 97.

† Synopsis of Difficult Parturition, p. 343.

Further, supposing we desired to prove, in regard to any one complication or disease, that some particular mode of treatment or of operation was more successful than another, we can resort to no other definite mode of decision than statistics; and no other known mode of investigation could yield the same simple and satisfactory results. In illustration, let me adduce an instance from obstetric surgery.—The normal conjugate diameter of the brim of the pelvis is four inches. Sometimes, however, it is morbidly contracted. When, as occasionally happens, this bony canal is so much diminished in size that its opposite walls are not more than from two and a half to three inches distant from each other, the mutilation and destruction of the child's head by craniotomy was formerly supposed, by British accoucheurs, to be the only proper, or indeed possible, mode of delivery. Latterly, after great and strenuous opposition, a revolution in practice has taken place in such cases; and, at the present day, the artificial induction of premature labour at the seventh month is the established rule of treatment. The child's life has thus certainly been saved, in many instances, from otherwise inevitable destruction. But is the life of the mother not placed, as has been sometimes alleged, in greater jeopardy by it? Statistical evidence answers the question by showing, that while craniotomy is fatal to the mother in about one in every five cases, the induction of premature labour is not fatal to her life in more than about one in every fifty cases. Nor would it be possible to place such a question beyond the possibility of doubt except by statistics, built upon a proper and sufficient basis of data. Mere impressions and arguments would not solve the problem. Dr. Osborne declared, for instance, that craniotomy was rarely fatal; and Madame Lachapelle described the induction of premature labour as rarely safe. Statistics have amply belied both opinions.

FIFTH PROPOSITION.—*Statistics offer a test by which the impressions of unrecorded and limited experience are corrected; and they furnish a mode of investigation capable of resolving many existing practical problems in surgery.*—It is only since statistics began to be applied to surgical investigations, that surgeons themselves seem to have become aware of the excessive rate of mortality accompanying most of their capital operations. As long as the mere impressions of cases upon the memory was depended upon, and the individual cases or facts themselves not noted or counted, the most erroneous opinions prevailed regarding the rate of mortality following upon surgical practice. All conclusions drawn from the memory are, observes Malgaigne, "horribly fallacious (*horriblement infidèles*), and it is," he adds, "to their employment that we owe

the astonishing delusions almost generally professed regarding the real danger or fatality of amputations."*

In his *System of Surgery*, Mr. Benjamin Bell, one of the most esteemed surgical practitioners and writers of his day, in some general remarks upon amputation, observes, "In the present improved state of the operation, I do not imagine that one death will happen in twenty cases, even including the general run of hospital practice: and in private practice, where due attention can be more certainly bestowed upon the various circumstances of the operation, the proportion of deaths will be much less."† But in 1844, Mr. Inman collected the statistics of 3586 cases of "amputations generally, including secondary, primary, for accident or disease;" most from hospital, but some from private practice, and he found that out of these 3586 cases, there died 1146, or 1 in every $3\frac{1}{10}$.‡ In a very valuable communication, Mr. Fenwick has published a collated table of 4937 amputations. Out of these 4937 cases 1565 died, or the operation was fatal in the proportion of 1 in every $3\frac{1}{5}$ submitted to it.§ The late statistical investigations of Phillips, Lawrie, Malgaigne, Gendrin. &c., have all fully borne out the same view with regard to the great mortality attendant upon amputations.

Nor are opinions formed from a single accurately observed case, or a very limited number of data, to be depended on as the ultimate probable measure of the value or fatality of an operation. An impression from an individual case often leads us to form a wrong estimate of the average danger or average safety of an operation, while adequate statistics at once show us the truth. Mr. Pott|| saw amputation at the hip-joint performed in one case, and from that case drew the general deduction, that in this operation the want of success would be uniform. Mr. Syme, after performing amputation at the hip-joint in one case, from that case drew the opposite deduction. "I firmly believe¶ (says he), that if the operation be done properly, and above all, quickly, its success will be general, if not uniform." In his excellent "*System of Surgery*," Professor Fergusson of London, depending upon his impression of cases, but not actually counting them, observes in regard to amputation at the hip-joint, "although no reasonable practitioner

* Archives Générales de Médecine for April, 1842, p. 391.

† A *System of Surgery*, by Benjamin Bell, vol. vii. p. 254 of 7th edition.

‡ *Lancet* for 5th October, 1844, p. 39.

§ *Monthly Journal of Medical Science* for October, 1847, p. 238.

|| *Chirurgical Works*, vol. iii. p. 217, 218.

¶ *Edinburgh Medical and Surgical Journal*, vol. xxi. p. 27. "Successful case of Amputation at the Hip-joint." The patient died "at the commencement of the eighth week from the operation."

would ever attempt it except as a last resource, it is somewhat satisfactory to know, that with all the disadvantages under which it has been performed, particularly in military practice, and notwithstanding the fearful shock that must of necessity attend such an extensive mutilation, the success of the operation has probably been such, that 1 patient out of every 3 on whom it has been performed has been saved.* But when we turn from such conclusions to the simple evidence of numerical facts, all of these three opinions are seen to be erroneous; and, in addition, the mind at once obtains a precise and definite idea of the degree of danger attendant on the operation when we simply appeal to statistics and find, as Mr. Sands Cox has shown,† that out of 84 cases in which amputation at the hip-joint has now been performed, 26 were successful, and 58 unsuccessful; or, out of every 10 operated upon, 7 died and 3 recovered.

Now the certainty and correctness of the knowledge which we obtain in this and similar instances from merely and simply *counting* up a hundred accurately recorded cases, is infinitely superior to a hundred separate opinions and arguments upon the matter. A hundred writers upon such a subject as the probable degree of fatality accompanying amputation at the hip-joint, would no doubt give us every variety and conflict of opinion on the subject. A hundred cases of it correctly noted and counted, would give us a result not admitting of any variety or conflict of opinion, except it were objected that the numbers on which the calculation was founded were too small for a perfect conclusion; and this objection can always be met by collating additional data in order to extend the basis of our calculation, and thus remove and free it from this chance of possible error.

Upon my own mind, the strongest conviction is impressed, that the numerical or statistical method of inquiry is yet ere-long destined to advance and promote surgical science, by revolutionizing some departments of surgery, by rectifying a number of its existing errors, by clearing away many of its doubts and difficulties, and by settling and determining for it definitely, various of those practical questions upon which the opinions of the best operators are constantly and ever changing. In the introductory remarks to his last work on surgery,‡ the

* *Practical Surgery*, p. 362. The rate of mortality which Mr. Fergusson here attributes to amputation at the hip joint, is in fact the rate of mortality belonging to all the *minor* amputations of the limb, taken as a whole, as has been shown in the preceding paragraph. The actual fatality of amputation at the hip-joint is above 2 in 3, instead of being 1 in 3.

† *British and Foreign Medical Review*, July, 1846, p. 112.

‡ *Institutes of Surgery*, p. 22.

late Sir Charles Bell remarked, "Men's opinions go to extremes; they vibrate like the pendulum." But the application of statistics to surgery will betimes impart greater precision, and accuracy, and stability to its opinions; for it forms, I believe, the simple, and, at the same time, the only possible means of deciding numerous doubtful and disputed questions in the practice of it. It has, for example, been much and long debated whether the circular or the flap method of amputation is the safer and the better mode of operating. The most opposite sentiments are still expressed, and the most opposite practices still prevail in reference to this point. Some surgeons and some surgical schools earnestly maintain the preferableness of the one method; and others as strongly uphold the greater safety and greater propriety of the opposite plan. Even the same mind, with every anxiety to arrive at nothing but the truth, may repeatedly change, and at different times hold different opinions upon the matter.* But the question at issue between the flap and circular methods of amputation is principally this;—which operation least endangers the patient's life? Now this question is one which could be satisfactorily settled by statistical investigation, and no doubt will ultimately be so. Probably one or two thousand amputations of the limbs are performed every year by the hospital surgeons of the United Kingdom. If our object were to ascertain whether amputation of the thigh by the flap or circular methods were the safer as regarded the *life* of the patient, and our hospital surgeons were only to note carefully and collect the results of this operation for a year or two, so that we should have the statistical returns of both operations and their results upon a sufficient number of unselected cases, we would thus become furnished with data, the mere counting up of which would show us (infinitely better than any argument) whether the two modes of amputation differed at all in their relative degree of fatality; and if they did differ, which was the most dangerous of the

* In the first observations which Mr. Syme published on amputation, (Edinburgh Medical and Surgical Journal, vol. xxi. p. 31,) he strongly maintained "that the circular mode of amputation is in every point of view bad;" and, writing in 1842, he still held that "amputation of the thigh ought always to be performed by making flaps."—(Principles of Surgery, p. 156.) In 1845, Mr. Syme believing, from statistical evidence, "that there is something radically wrong in the principle" of amputation of the thigh, both by the flap and circular methods, proposed in their stead amputation at the knee; and thus dividing the thigh-bone through its condyles, instead of through its shaft.—(See Monthly Journal of Medical Science for May, 1845, p. 337.) In the same Journal for November, 1846, (p. 225,) he does "not persist in advocating amputation at the knee," but avows himself now satisfied that the old circular method of amputation may be "employed at the lower third of the thigh safely and advantageously," "and should be preferred to the flap operation at a higher part of the limb, when the circumstances afford room for choice."

two; and what was the degree of the comparative excess of danger of the one over the other. By the same form of statistical inquiry upon the same or other cases, and by analysing or decomposing each separate case into as many parts as it contained distinct objects, we could further ascertain and determine all the various minor points, such as—which operation required the shortest period of convalescence—which ultimately secured the best form of stump, &c. &c.

And here I would beg to add one observation relative to the probable future importance and bearing of statistics upon surgery. It will be found that, in most of the past literature of their profession, surgeons have almost invariably contented themselves with recording their own deductions from their own cases, without recording the cases themselves. They have left us their inferences, but have not left us the grounds and bases of these inferences. They have generally given us, not their individual cases or individual facts, but the opinions which they themselves thought fit to draw from these facts.* The result has been, that, in numerous instances, inferences of the most erroneous and contradictory kind have been drawn, in consequence merely of the elementary facts observed and generalized upon, being far too few for the establishment of a correct deduction. I have already offered an instance of this in the two opposite opinions expressed by Mr. Pott and Mr. Syme regarding amputation at the hip-joint, from single cases observed by each, as contrasted with the actual and ascertained degree of danger connected with that operation. A sufficient series of individual facts, collected from the practices of several different surgeons, may thus point out a deduction quite at variance with the so-called experience and opinion of the individual authors themselves. Few surgeons allow that strangulated hernia, when properly operated upon, is very fatal in its results. "The operation," says Mr. Pott, "if applied to in time, very seldom fails; so seldom, that I believe I might venture to say, *not* 1 in 50 dies of it if timely and judiciously executed."† But, out of 77 cases recorded in Sir Astley Cooper's work on hernia, 36 died; out of 183 operations for this disease, collected by Malgaigne, 114 proved fatal; out of 545 collected by Dr. Inman, 260 died; or 1 in every 2 perished instead of 1 in 50. Again, amputation of the thigh is fatal in nearly the same proportion; or about 1 in every 2 or 3 who are subjected to it dies. Out of 987 cases of this operation collected by

* In the past records of midwifery, on the other hand, we have all their important *individual* facts and cases left on record for us, in the works of Mauriceau, Portal, Giffard, Smellie, &c., &c.

† Chirurgical Works, vol. ii. p. 180.

Mr. Phillips,* 435 of the patients, or 1 in every $2\frac{2}{10}$ perished. Yet, speaking of the degree of danger accompanying amputation of the thigh, as apparently inferred from his own observations upon the point, Mr. Ormerod, in a late work on surgery, observes that, "considering the severity of the operation, and extent of the injury done to an individual by the removal of a limb above the knee, the success of the amputation there is very great; the patients are often placed under bad circumstances for operation, and their health very much reduced, yet death from failure, after the removal of a limb for disease, is *very rare* indeed."† I repeat, that if, instead of dealing in vague and valueless generalizations and opinions of this stamp, surgical authors would only carefully note and record all their individual operations and results, with a view that betimes a sufficient collection of data might be thus gradually gathered together, in order to settle particular questions of surgical science or practice, then their collated facts would in determining such questions, be indescribably more valuable than their individual opinions. For the facts and testimony of surgery would thus become *cumulative*, and increasingly conclusive upon any points on the investigation of which it was deemed proper to direct the power of its evidence, instead of being lost and frittered away, as at present, on the formation of a host of isolated opinions, which are too often not less perplexing from their contrariety than from the confidence and dogmatism with which they are severally advanced. It is interesting to reflect how much, in all of these respects, might soon be accomplished by proper and systematical annual reports from the great public surgical hospitals throughout the country; and at the same time it is distressing to consider what masses of valuable information are yearly lost from the mere want of such reports.

SIXTH PROPOSITION.—*Statistical Evidence alone enables us to ascertain correctly the effects of various minor conditions upon the Fatality of Operations,—such as the influence of the age, sex, &c., of the patient; the special success of different operators, &c.* The results of surgical operations are, like the results of diseases, varied by age, sex, constitution, idiosyncrasy, &c. On the influence of these, and other minor points, some surgeons may have been led to form and express opinions more or less correct; but it is only by employing the numerical or statistical method of examination, that a perfect degree of accuracy of judgment can be possibly attained on such matters.

* Medical Gazette for 1844, p. 805.

† Clinical Collections and Observations in Surgery, p. 135. London, 1846.

Without statistics, all opinions on these points would have remained doubtful and undetermined; by statistics, their influence can be at once discovered and measured, and that, too, by a kind of evidence which is at once simple and convincing. I shall adduce one or two points as an example of the whole.

Let us take the influence of *age* upon the results of the operation of lithotomy. Various late authors have published the ages of their patients, and reports, which might be reduced to show the influence of age upon this operation. I shall content myself with tabulating, for this purpose, the earliest of the kind ever published; viz., those of Cheselden. He has left records of the ages and results of lithotomy, in 213 cases operated upon by him at St. Thomas's Hospital.* Out of these, only 20 patients died, or the small number of 1 in $10\frac{1}{2}\frac{3}{5}$. But I shall throw all the 213 cases into a tabular form; and it will be at once seen, from this view of Cheselden's recorded data, that the danger of lithotomy increases in a ratio progressive with the age of the patient.

No. V.—*Table showing the influence of the Age of the Patient upon the Mortality of Lithotomy.*

Ages of the Patients.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per-centage of Deaths.
Under 10 years,	105	3	1 in 35	3 in 100
From 11 to 20 years,	62	4	1 in $15\frac{1}{2}$	6 in 100
From 21 to 40 years,	22	5	1 in $4\frac{2}{3}$	22 in 100
From 41 to 80 years,	24	8	1 in 3	33 in 100

Let us take another illustration from lithotomy of the capability of statistics, proving one more of these minor points, such as are alluded to in the general proposition. Without statistics, it would be difficult or impossible to demonstrate the influence of the mere *size* of the stone extracted upon the results of the operation of its extraction. But by statistics it can be readily proved that the mortality of lithotomy rises higher and higher in proportion as the stone increases in size; and hence, in all probability, in proportion as the operation increases in severity and difficulty. The following table, calculated from

* Cheselden's Anatomy, p. 332.

From the admirable researches of Mr. Edmonds and Mr. Farre, we know that the mortality of disease *in general*, and the mortality of *individual* diseases (as small-pox, &c., &c.), increases from puberty upwards, in a regular geometrical progression, and that the rate of increase is about three per cent. every year, or more nearly 34 per cent. every 10 years. I believe, that an adequate collection of data will very probably show that this same "constant" mortality regulates the degree of liability to death in lithotomy, amputation, and other surgical operations.

the Norwich data furnished by Mr. Crosse, in his valuable work,* affords the required numerical evidence for this generalization.

No. VI.—*Table of the mortality of Lithotomy, calculated according to the different Weights of the Stone extracted.*

Weight of Stone.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per-centage of Mortality.
2 oz. and under	648	65	1 in $9\frac{23}{83}$	10 in 100
From 2 to 4 oz.	46	23	1 in 2	50 in 100
From 4 to 7 oz.	9	5	1 in $1\frac{1}{2}$	55 in 100

Statistics in surgery have been objected to on the ground, that in combining cases, in order to arrive at a general result, we do not take cognizance of the superiority of the practices of individual operators. "We find (argues Dr. Bennett) operations by different surgeons, and various experiences, all mingled together to produce one sum total.† The most skilful metropolitan surgeon is put on a par with the country practitioner, and the experience of long practice is of no more value than that of the tyro. It is well known that, even in one person's practice, he operates differently at different periods of time. Mr. Syme had well illustrated this with regard to lithotomy, and told us that, since his alteration of the method of its performance, his success had been much greater than formerly. Yet, according to the reasoning of Dr. Simpson, all the operations must be added together, and those performed during the inexperience of youth and the senility of advanced life, must, as with Mr. Martineau, be put on the same level with the cases that form the boast of mature age, and the most perfect powers of mind and body."‡

Now, in this as in other points, I believe that the statistical method of inquiry forms the means, and the *only* means, of enabling us to prove the very items which it is alleged that statistics lose sight of and conceal. Take, for instance, the identical examples adduced. The present *general* average mortality of lithotomy, as performed by *all* operators on subjects of all ages, is, according to Dr. Willis, about 1 in 8.§

* Crosse.

† Of course this is necessary when we wish to ascertain the *general* average success of an operation in the hands of *all* surgeons, and not its *special* average success in the hands of any *individual* operator or operators.

‡ Monthly Journal of Medical Science, October, 1847, p. 307.

§ "The average mortality from lithotomy, on all hands, appears at present to be about one in eight."—Dr. Willis's *Urinary Diseases*, 1838, p. 347.

Out of 5900 cases collected by Mr. Inman, 765 patients died, or 1 in $7\frac{3}{4}$. Out of 14 cases operated upon by Mr. Syme, and recorded in his surgical reports in the Edinburgh Medical and Surgical Journal (vol. xxxiii. to vol. xxxix.), 5 died, or 1 in $2\frac{2}{3}$. Since adopting his present plan of lithotomy, however, he had performed 17 operations in the hospital, of which 2 only have proved fatal, or 1 in $8\frac{1}{2}$. Now, this difference could not be deduced or stated with accuracy in any other way than by figures, or by the statistical method; for by it alone can we determine the *special* averages of different operators, or of the same operator at different times. But "take care (observed Sir Astley Cooper) how you draw any deduction from particular cases. I and many others have for a length of time met with extraordinary success in operating for the stone, when 4 or 5 unsuccessful cases in succession have come, which have generally brought down the result to the amount I mentioned, viz., that 2 in 15 die."* Mr. Martineau's practice afforded a curious illustration of the necessity of this caution.

In the 11th volume of the "Medico-Chirurgical Transactions of London," Mr. Martineau published an account of 74 cases, in which he had performed the operation of lithotomy in the Norwich Hospital from the year 1804 to 1840.† Only 2 of these 74 died, or 1 in 37. We learn further, however, from a paper of Dr. Yelloly,‡ that Mr. Martineau operated in the same hospital on 73 additional cases (147 in all). Out of these 73 additional cases, 15 died, or 1 in $4\frac{9}{10}$. And I repeat, that it is statistics only which could properly and fully prove to us this great *special* difference in the success of Mr. Martineau's practice at different periods. At the same time, however, the same case proves to us further, that if we wished to obtain not this *special* average of practice at a *selected* time, but the *general* average of all his practice at all times, it would amount to nearly the general average of most other operators. For out of his whole 147 cases, 17 died, or 1 in 8, which we have seen to be nearly the common degree of success in lithotomy, according to the investigations of Drs. Willis and Inman. The special average success of some operators has been greater than this. We have already seen that Cheselden, out of 216 recorded hospital cases, only lost 20, or 1 in $10\frac{1}{3}$. The special average success of other operators has been less. Out of 356 Parisian cases collected by Dupuytren, 61 died, or 1 in 6.

* Lectures on Surgery, p. 321.

† He records 84 cases in all, with 2 deaths; but 10 of these 84 cases had occurred in private practice.

‡ Philosophical Transactions for 1829, p. 63. "The whole number of Dr. Rigby's operations [in the Norwich Hospital] was 106, with 15 deaths; and of Mr. Martineau's 147, with 17 deaths."

Such differences, I repeat, could never be proved and substantiated, unless by statistics.

SEVENTH PROPOSITION.—*Statistics afford us in general the only true and ultimate "measure of value" of any proposed alternative operation, or of any new practice in surgery.* Every well-informed writer has naturally and almost instinctively recourse to this form of proof, when originating a new, or reviving a neglected operation. For example, in his able treatise on "Excision of Diseased Joints," Mr. Syme, in recalling the attention of surgeons to the operation of Park and Moreau, and in showing the advantages and safety of excision of the elbow joint, as compared with the alternative operation of amputation of the arm, most properly uses the following statistical argument: "I have," he remarks, "cut out 14 elbow-joints, and the operation has been performed in Edinburgh three times by other practitioners; of all these 17 cases, only 2 have terminated fatally; and in one of them the patient would, I believe, have died from any operation whatever, while, in the other, the disease was found so extensive as to render the excision almost impracticable. I believe the result of 17 amputations in similarly unfavourable constitutions would not be so satisfactory."*

Anatomical and pathological or other considerations may suggest to us the propriety or impropriety of any newly proposed condition or mode of operating; but an appeal to statistics is the only means of ultimately and definitely deciding upon its merits or demerits. For instance, surgeons were long afraid to place a ligature upon the carotid artery, fearing the difficulties of the operation, and the probabilities of danger to, and derangements in, the cerebral circulation. Statistics, however, show that this reasoning was so far unsound. Ligature of the carotid has now been performed above two hundred times.† Out of that number it has proved fatal in the propor-

* Treatise on the Excision of Diseased Joints, p. 26.

† I have collected the following table from an elaborate essay of Dr. Norris, of Philadelphia, on Ligature of the Carotid, &c., published in the 27th Number of the American Journal of the Medical Sciences.

Table of the Statistical Results of 203 Cases of Ligature of the Carotid Artery.

Cause for Operation.	No. of Cases.	No. of Deaths.
Aneurisms	38	16
Wounds, &c.	30	15
Extirpation of Tumours	18	6
Cerebral Affections	6	0
Erectile and other Tumours, &c.	42	13
Brasdor's Operation	15	4
Total	203	54

tion of about 1 in every 4 patients operated on; and consequently it has become an established operation in surgery,—many capital operations being more mortal than this.

After seeing the comparative safety with which ligature of the carotid and other large arteries was performed, surgeons reasoning on these results, believed it would be justifiable to tie the arteria innominata. The results, however, have belied the *a priori* reasoning. Ligature of the arteria innominata has now been performed, according to Dr. Norris, in eleven cases. All the eleven patients died.* In three other cases, occurring in the practice of Post, Key, and Porter, the operation was commenced, but not completed. Two of these three patients died. In the third (Mr. Porter's) the idea of tying the artery was abandoned after it was exposed; the wound was closed up, and the patient recovered. And, doubtlessly, these statistical results will, ere long, compel surgeons to acknowledge this operation to be one which it is unjustifiable in them to practise.

Every newly-proposed practice in surgery thus offers, as it were, a new problem for statistical solution. In the instance of etherization a small number of cases might be sufficient to satisfy any unprejudiced mind, that the etherized state was one in which the patient was saved from the pain of the surgeon's knife; but a small number of cases could not prove—any more than abstract reasoning could prove—whether etherization were, or were not, a safe practice as regarded the probabilities of the ultimate recovery of the patient after severe operations; whether, that is to say, it increased, diminished, or altered in any respect these probabilities. And hence the origin of the present communication. But before attempting to show the solution of this problem by statistics, let me premise in another proposition an answer to the chief objections which have been urged against the method of inquiry that I have adopted.

EIGHTH PROPOSITION.—*The objections of late years offered against the application of statistics to practical inquiries in surgery and medicine, seem altogether founded upon a misconception of the objects and principles of statistical investigation.*—The application of the statistical or numerical method of inquiry to the solution and determination of questions in medical and surgical science, is of comparatively late date.† Like most other innovations, its introduction has been more or less

* The operations in these eleven cases were performed by Mott, Graefc, Hall, Dupuytren, Norman, Bland, Lizars, Hutin, Arendt, Liston, and Kuhl.

† See some historical notes regarding it in a paper by Mr. Marshall, in the Edinburgh Medical and Surgical Journal, No. 116.

strongly opposed;* and the principal objections which have from time to time been urged against the employment of it, have been the following:—

1st Objection:—The numerical or statistical method consists of a calculation of probabilities. There is no doubt whatever of the truth of this allegation. But if it formed a valid objection against the application of statistics to medicine and surgery, it would form equally a valid objection against almost all other modes by which the human mind struggles to acquire increased knowledge, either in medicine or in any other department whatever of science and art. For, as the great French mathematician Laplace observes,—and I could not quote a greater and higher authority on such a point,—“To speak strictly, almost all our knowledge is but probable; and among the small number of things which we can know with certitude, in the mathematical sciences themselves, the *means* to arrive at truth are founded upon probabilities; so that the *entire* system of human knowledge is subjected to the theory of probabilities.”†

2d Objection:—The numerical method calculates together as similar, facts which are not sufficiently similar to be a basis of calculation of probabilities. M. Double and others who have propounded this objection, have affirmed, that no two cases in medicine or surgery are entirely or exactly the same, and hence that they cannot be counted together as the same. But if this strange and illogical averment were true to the extent to which its supporters maintain, and if every single case to which the physician was called, and every single operation which the surgeon performed, were an individuality and unity so dissimilar from all other previous cases of disease or operation which he had witnessed, as to be incapable of being grouped or classed in any way with them,—then we could not possibly have any general facts, principles, or rules to guide us in the practical exercise of our profession. And, if medicine and surgery had no such general laws or principles, there would be necessarily an end to their existence, either as sciences or arts. Grant, however, that there are specific diseased states in medicine, and specific operations in surgery, with some general facts or rules applicable and peculiar to each

* Our semi-civilized brethren of China, with their fixed hatred of all improvements and innovations, seem to have a particular dislike to statistics, and upon grounds amusingly similar to those of some members of the medical profession in Europe. “Moreover (says Mr. Fortune), they [the Chinese,] cannot appreciate statistical inquiries; they always fancy we have some secret motive for making them; or that the subject cannot be of the slightest importance either to ourselves or others.”—*Three Years’ Wanderings in China*, p. 3.

† *Essai Philosophique sur les Probabilités*, p. 1, &c.

disease, and each operation; then, it is further evident, that each of these general facts must have been originally founded upon a basis or deduction of particular facts,—that the fundamental particular facts must be always *more or fewer*—consequently capable of being counted,—and, consequently, coming within the range of the numerical method of inquiry.*

But it has been further objected, allowing that individual cases of disease may be arranged into groups or species, still the individual cases composing these groups are often so inaccurately *observed* as not to form a sufficiently true basis for statistical comparison and inquiry. This objection, however, applies to all other modes of medical investigation as well as to the numerical. There is precisely this very same difficulty to overcome, in whatever way or by whatever method we attempt to study and generalize upon diseases. There is this difference, however—the statistical method compels and exacts *more* care, and caution, and correctness in our study, and in our records of cases than other plans of generalizing; and, certainly, *this* forms an argument in favour of the adoption of statistics, rather than an argument in favour of the rejection of them. For it is an acknowledged truth, in medical as in all the other sciences, that the greatest attainable degree of accuracy in our fundamental or elementary facts is necessary, that we may reach the greatest attainable degree of accuracy, and consequently of utility, in the general practical conclusions or laws which we venture to deduce from these facts. From time to time we are obliged, in *every* known form of medical reasoning and generalization, to revise our fundamental facts, and change or modify our conclusions as our knowledge of pathology, diagnosis, &c., increases. The same holds true of the numerical method. And at present, the principal obstacle against applying statistics, more fully than has been done, to some departments of the physician's study, confessedly consists in our occasional inability to make a perfect and undoubted diagnosis of some *internal* diseases, and hence in the liability of our comparing and calculating together cases that are not

* If the reasoning of M. Double were admitted, "it would," as Gavarret properly observes, "altogether strike down medicine from the position which it ought to occupy in the temple of human knowledge. What language," he adds, "can a physician address to his pupils, who will not see, anywhere, but individualities? On what ground can he recommend them such or such treatment for their patients, since they ought never to meet, in their practice, anything comparable with what their master has seen? According to this inadmissible hypothesis, medical experience would be a word without meaning; the student, who has never yet seen a patient, would necessarily know as much as the most perfect physician. For if the career of the latter, and his predecessors, is consumed in the sterile observation of a succession of therapeutic individualities, the healing art cannot but be composed of a series of isolated attempts, without a common tie, and from which it would be impossible to draw any general conclusion, or any precept for the future."—*Principes Généraux de Statistique*, p. 42.

specifically similar. In statistics, however, as applied to surgery and surgical questions, the same obstacle does not meet us, at least in the same degree. Supposing we wish, for instance, as will subsequently be my object in the present investigation, to calculate what proportion of persons dies under particular amputations,—we only require perfect accuracy on three facts, each and all of which could certainly be noted, and, by a little care, noted with perfect accuracy, and without much, if indeed any, chance of error. For they amount to these points in each case, viz. : 1. Was amputation performed ? 2. In what part of the limb was it performed ? and 3. After its performance, Did the patient live, or did he die ? In this and many other points of surgery, to which the numerical method of inquiry is capable of being directed, all the necessary elementary matter could assuredly, with any common attention and accuracy, be readily collected without much probability or possibility of error. Of course, it is unnecessary to add that in this, as in all other modes of philosophizing, our observations and deductions must be pursued with stern and strict honesty, and with a view to the attainment of truth, and truth only ; and that, for this purpose, *all* the pertaining individual facts or cases must be always given, and always counted. There must be no omission ; no concealment ; no selection of any kind.

3d Objection :—The statistical method of inquiry is different from and opposed to the inductive method.—No opinion could be more erroneous. The numerical method is assuredly not opposed to that strict observation of individual facts, and that strict generalization of them, which constitute the double basis and essence of the inductive method ; but the very reverse. 1. It demands in the same way the strict observation of individual facts ; but it demands that the observation and record of them be made, if possible, with still greater care and accuracy than heretofore. 2. It educes in the same way the general principles or laws of practice from the comparison and analysis of these observed facts ; but it enforces more rigorous accuracy than heretofore in the deduction of these principles or laws, in proportion as figures are more certain than memory, and actual enumerations more certain than general impressions. Its required mode of observation and mode of generalization are the same as in the common inductive method, only more rigid and hence more rigorously correct. Its object is not to supplant but to supplement our former methods of inquiry,—not to make them useless, but to make them more useful by making them more accurate. It is an instrument which enables us to draw our deductions, not only with greater simplicity, but also with greater truth and precision. “No man (says Bacon), be

he ever so cunning or practised, can make a straight line or perfect circle by steadiness of hand, but this may be easily done by help of a rule or compass."* And those who maintain that the numerical method is different from and opposed to experience and induction, might as rationally argue, that "when we try to ascertain the general fact of the rapidity of a patient's pulse, we employ two different methods when we attempt to attain the desired information *without*, and *with* the aid of a stop-watch. We here practise two methods that are not different or opposed to each other. They are logically the same in all respects. But the one method is relatively more accurate, precise, and determinate than the other. And in this as in other applications of the numerical or statistical method, the language we employ becomes at the same time simple and decisive. For if we say, for instance, that the beats amount to 100 a minute, our language is far more clear and simple than if we mentioned that the pulse was "*quick*," or "*rapid*," or "*frequent*," &c. We state a definite and intelligible fact, instead of using some comparatively indefinite and uncertain term which the very temperament of every speaker and hearer may interpret differently. The great aim and object of the statistical or numerical method of inquiry, as applied to surgery and medicine, is the determination of their general facts, or general laws, with the utmost attainable degree of accuracy; but, in truth, figures are not only the strictest and most correct way of *educing* their general facts,—they form also the shortest and most correct way of *stating* or *expressing* them after they are educed.

TABLE VII.

Shows the Mortality following Amputations of the lower extremity, performed for diseases and accidents, in different Hospitals.

	THIGH.						LEG.					
	Pathological.			Traumatic.			Pathological.			Traumatic.		
	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.
Liverpool Northern Hospital, ...	19	1	19	10	3	3.33	13	3	4.33	27	6	4.5
Glasgow Infirmary, (Lawrie), ...	92	19	4.84	36	27	1.33	35	12	2.91	27	18	1.5
Chester Infirmary, ...	9	2	4.5	4	3	1.33	2	.	.	4	4	1
Mass. and Penn. Hospitals, ...	28	5	5.6	22	10	2.2	20	3	6.60	29	9	3.22
Hotel Dieu (Roux), ...	12	5	2.4	4	4	1.35	9	2	4.5	3	3	1
M. Malgaigne, ...	153	92	1.66	46	34	1.59	113	55	2.05	79	50	1.58
Mr. Alcock,	43	27	1	.	.	.	11	4	2.75
Lyons Military Hospital,	4	3	1.33	.	.	.	5	3	1.66
Paris do,	5	1	5	.	.	.	5	3	1.66
Royal Berks Hospital,	1	.	.	3	.	.
Total, ...	313	124	2.52	174	112	1.55	193	75	2.57	193	100	1.93

* See Bacon's Advancement of Learning, in Montague's edition of his Works, vol. ii. p. 182.

TABLE VIII.

Shows the Mortality following Amputations of the upper extremity, performed for diseases and accidents, in different Hospitals.

	SHOULDER JOINT.						ARM.						FORE ARM.					
	Pathol.			Traum.			Pathol.			Traum.			Pathol.			Traum.		
	No.	Dead.	Av.	No.	Dead.	Av.	No.	Dead.	Av.	No.	Dead.	Av.	No.	Dead.	Av.	No.	Dead.	Av.
	Mort.			Mort.			Mort.			Mort.			Mort.			Mort.		
Liverpool N. Hospital,.....	1	1	1	1.	1	1	7.	10	3	3.33	1
Glasgow Infirmary,.....	2	1	2.	4	2	2.	17	3	5.66	36	18	2.	4	18
Royal Berks Hospital,.....	1	2	1	3	4
Chester Infirmary,.....	2
Massachusetts and Pennsylv. Hospital,.....	1	1	1	1.	4	11	2	5.5	5	16	1	16.
Hotel Dieu,.....	1	2	1	2.	2	2
M. Maligne,.....	7	4	1.75	5	7	1.	61	24	2.54	30	17	1.76	17	5	3.4	11	3	3.66
Mr. Alcock,.....	10	2	5.	38	19	2.	9	4	2.25
Lions Milit. Hospital,.....	2	2	1.2	7	6	1.16	1
Paris Milit. Hospital,.....	2	1	2.	4	2	2.
Total,.....	12	5	2.8	29	15	1.93	91	28	3.25	123	69	1.92	38	5	5.6	68	8	8.5

Unless we altogether refuse our belief in facts, no doubt can remain upon this subject—no case can be more completely proved; with the exception of the fore-arm, we find the results of the pathological amputations everywhere more favourable than those of the traumatic.* In one hospital only one death occurred in 19, after removal of the thigh for disease; whilst for accidents the mortality was as high as 1 in 3.33: in another, after amputation of the leg, 1 out of 4.5 perished when it was performed for pathological causes, and out of 3 amputations required for diseases all died. Again, if we examine the average success of all united, the same difference is apparent; thus, in amputations of the thigh for disease, 1 dies in 2.52, and after those of the leg 1 in 2.57; whilst 1 dies in 1.55 in the former operation, and 1 in 1.93 in the latter when required for accidents.

But a still more remarkable circumstance appears, viz., that, in calculating the chances for a patient, it is necessary rather to consider the cause for which the operation is performed, than the limb which is removed. Thus, although we have before proved that a vast difference exists between the results of amputations of the thigh and those of the removal of an arm, yet here we discover the mortality to be less in the former when required by disease, than in the latter when undertaken for accidents: in the one case we have a mortality of 1 in 2.52;

* Although, perhaps, not strictly correct, yet, in accordance with the phraseology of other writers, I have used the terms pathological and traumatic amputations, as designating the amputations performed, in the former case for diseases, and in the latter for accidents.

in the other, 1 perishes out of 1·92. It is difficult of explanation why this rule is reversed in the fore-arm, the mortality, both at the Newcastle Infirmary and at Paris, having been greatest in the pathological amputations: in the former hospital, 1 out of 6 died after the amputation performed for injuries, and 1 in 3 when the operation was performed for disease; and in the hospitals of Paris, Malgaigne gives the mortality for pathological amputations performed at this part at 1 in 3·4, whilst in the traumatic class it was 1 in 3·66.

In order to throw some light upon the causes from which the difference of mortality between pathological and traumatic amputations arises, the following table has been added.

TABLE IX.

A comparison of the Mortality observed in the Newcastle Infirmary, at different periods, after Amputations performed for accidents and diseases.

NEWCASTLE INFIRMARY.

	Within 4 days.	4 to 7 days.	2 wks	3 wks	4 wks.	5 wks.	6 wks.	7 wks.	9 wks.
Injuries of thigh.....	2·66	..	5·	4·
" leg.....	9·5	..	8·5	7·5	26·	25·
" arm.....	8·	21·
Amputation of shoulder-joint.	2·5
Diseases of thigh and knee,...	25·	48·	23·5	..	15·	42·
" leg and foot.....	74·	36·5	14·2	33·	64·	63·	62·	61·	60·
" arm and elbow,...	11·
" fore arm and wrist	9·	8·	7·

During the first four days, or, in other words, during the period of shock, the amount of death is ten times more in the thigh amputations, and eight times more in the leg amputations after an accident, than when these operations are performed for disease. Whilst none had died of 74 from shock, exhaustion, or delirium, in the pathological amputations, 12 had perished out of 106 who had submitted to the operation for accidents, or 1 in every 8·83. Again, with respect to gangrene of the stump, which we before remarked as depending on shock, and which, we said, had usually its fatal termination during the first four days, although the average mortality had been 1 in 17·66 after amputations for injuries, no death had occurred from it amongst those previously affected with long-standing complaints.

Secondary hemorrhage, on the contrary, is most fatal after amputation for diseases, few persons undergoing an amputation for an accident being cut off by it. It will be observed, that the cases which are stated to have died from it after traumatic amputation, were both in the class of the secondary operations, so that no person suffering a primary amputation had sunk from its effects. It is probable that this

arises from the greater debility of the patients not permitting the system to rally after the loss of blood; for the occurrence of secondary hemorrhage is not much more frequent amongst amputations for diseases than for accidents. Thus, in Mr. Alcock's returns, 1 case out of every 6.3 was affected with it after primary amputation; whereas in the secondary class, which may be said to be composed of persons worn down by disease, 1 out of 4.72 was attacked; but of the former class, whilst only 1 in 57 died, in the latter 1 out of 13 perished; and in four other cases of secondary amputations, the depression produced by it assisted in destroying the patient; so that, whilst in the primary amputations an unfavourable effect was produced in only 1 in 57 cases, in the secondary, either death or a very dangerous state of depression was observed in 1 in every 6.5.

Erysipelas and visceral inflammations, are by far more frequent in amputations for accidents than in those performed for diseases. In the pathological class none had died from the former cause, and only 1 in 24.6 from the latter; but in the traumatic operations 1 out of every 26.5 had perished from the one, and 1 of every 10.6 of the other. It will be also observed, that as both classes are pretty nearly equal in their liability to phlebitis, the chief excess of mortality, in the second and third weeks after amputations for injuries, is produced by inflammatory complaints. From this the practical rule may be deduced, that, after amputations for injuries, we should most carefully regulate the diet of the patients, confining them, in the majority of instances, to an antiphlogistic regimen for the first three weeks, at least, after the operation, and that constant care should be employed to catch the first symptoms of inflammatory disease.

After the fifth week, according to Table IX., although no cases of traumatic amputation had been cut off at the Newcastle Infirmary, several instances of death appear to have occurred amongst those operated upon for diseases; and it was found that the only case of bed-sore belonged to this latter class. Another cause of death besides exhaustion is, that in some cases of pathological amputation, diseases of important viscera, as phthisis, which had previously been latent, become quickened into activity, and destroy the patient after the removal of the limb.

CHAPTER VI.

DOES ANÆSTHESIA INCREASE OR DECREASE THE MORTALITY ATTENDANT UPON SURGICAL OPERATIONS?

"Why dost thou whet thy knife so earnestly?

Shylock must be merciful.

On what compulsion must I? Tell me that."

Shakspeare's Merchant of Venice.

IN two papers on Etherization in Surgery, published in the *Monthly Journal of Medical Science* for September and November, 1847, I took occasion to discuss various points connected with the subject, and more particularly dwelt upon the necessity of having recourse to the evidence of a large collection of statistics as the only proper and legitimate method of determining the fact, whether the previous superinduction of artificial anæsthesia increased, decreased, or altered in any way the mortality attendant upon surgical operations. During the intervening period, various circumstances and engagements have intervened to delay the publication of the following inquiry, the results of which were laid at length before the Medico-Chirurgical Society of Edinburgh in July last. From that time up to November, I continued to receive additional returns, all of which have been embodied in the Tables, pp. 72, 73, and 74.

Shortly after etherization began to be employed in surgery, its alleged beneficial or baneful effects were keenly discussed among the members of the profession; and principally, or entirely, upon the results of individual or isolated cases. Some eagerly and stoutly doubted, *in toto*, the possibility of making operations painless; and many who admitted its possibility, denied altogether its propriety, on the alleged ground of its increasing the general subsequent dangers of the patient, inducing a variety of alleged morbid states and lesions, and adding, on the whole, to the fatality of operative surgery.

Amidst the many conflicting and contradictory assertions that were uttered on these points, I became convinced that there was only one method of arriving at the truth, viz., by instituting a statistical investigation upon as large a scale as possible into the results of the practice, and thus ascertaining whether, out of an extensive series of operations performed

with and without etherization, the mortality was greater or was less when the patients were operated on in a narcotized and anæsthetic state, than when they were operated on in a waking and æsthetic state.

The first difficulty to be encountered in such an inquiry was the difficulty of obtaining a proper field and standard for the proposed comparison. But *first* of all it was evident that the comparison, whatever it might be, could only be properly instituted between patients operated on in public hospitals, with and without etherization. For we had nowhere published, nor did it seem possible to obtain, any adequate comparative returns of the results of operations from the surgical practice of private practitioners. Besides, hospital returns were preferable in this respect, that there existed on the whole, everywhere, undoubtedly a far greater uniformity between the hygienic and other collateral circumstances of patients operated on in hospital than in private practice. *Secondly*, however, it was further evident, that in seeking and fixing upon a criterion by which we could compare the statistical results of surgical operations formerly performed without ether, with those now performed upon etherized patients, it was improper and impossible to institute the comparison between *all* operations and reports of operations in hospitals; for the severity and danger of the operations performed in, and reported from, different hospitals, differed immensely in their nature, and consequently in their results. In order, therefore, to obtain the primary requisite for a correct statistical inquiry—of having data of a *similar* kind and character for the proposed testing and comparison—it was necessary to select and contrast the results of some *one* operation without ether, with the results of the same one operation with ether. With this view I selected the larger amputations of the limbs as the fittest field on which to conduct the proposed investigation; and I restricted myself to hospital amputations of the thigh, leg, arm, and fore-arm, on account of their being everywhere performed in almost the same manner, for the same causes, under the same circumstances, and on the same class of subjects; and because there already existed extensive published researches, by Phillips, Lawrie, and Malgaigne, into their absolute mortality, when performed under ordinary circumstances and without anæsthesia, to aid us in satisfactorily determining the nature of the results of the new practice of operating upon patients in an anæsthetic state.

Having thus fixed upon the mode of inquiry, I proceeded to apply for returns from all the surgical hospitals of Great Britain and Ireland that I could hear of, as likely to have employed etherization in amputations. And I feel it quite impossible to

return thanks, in any adequate terms, for the very great politeness and kindness with which my inquiries were answered on all hands.* In some hospitals ether had not been tried, and I was consequently furnished with no data; in others in which it was used, my correspondents were quite at issue about its propriety; many were doubtful; some expressed themselves strongly against it and others strongly for it. But I was principally anxious to obtain the total results, believing that *they* would decide the question far more certainly than any individual ex-

* In my letter of application I stated, that "the effects, whether favourable or unfavourable, of etherization upon the ultimate recoveries of patients from surgical operations is still a matter of much doubt and uncertainty. We have as yet had no proper collection of data to ascertain whether the mortality of operations has been increased or not by patients being placed under the influence of ether at the period of their performance. In order to determine as far as possible this important point, I have been induced to undertake the statistical investigation of the results of the larger amputations in cases where ether inhalation was employed at the time of operation. Amputations have been selected for this purpose in preference to other operations, because they are, under all common circumstances, nearly and everywhere alike, and because the general average mortality accompanying most of the greater amputations is already known from the inquiries of Phillips, Lawrie, and others, and thus a ready standard of comparison is afforded us. You would, therefore, oblige me by filling up the following table with any results, however few in number, of amputations in which ether was used in your hospital. I especially wish to know *all* the deaths as well as all the recoveries in these operations; and by thus collating, on the whole, a large body of statistical data, I hope to be able to arrive at the same general results."

Copy of Form of Table sent.—"Results of Amputations performed upon Patients in an Etherized State in the ——— Hospital."

Seat of Amputation.	Primary or for Injury.		Secondary or for Disease.	
	Total No. of Cases.	Total No. of Deaths.	Total No. of Cases.	Total No. of Deaths.
Amputation of Thigh . .				
Amputation of Leg . . .				
Amputation of Arm . . .				
Amputation of Fore-arm .				
Total				

It may be proper to remark, that in answer to the returns, I had the results of twenty-four amputations of the fore-arm sent me, which are not included in the subsequent remarks in the text. Out of these twenty-four amputations ten were primary, with one death, and fourteen secondary, with two deaths. I have omitted them in the text, in consequence of finding that Mr. Phillips, in his standard of amputations, confines his returns to those of the thigh, leg, and arm, and does not include those of the fore-arm.

perience or individual opinion could. In Table No. I (see p. 72 and 73), these results are given in a detailed form, with the names of the gentlemen who kindly reported each return to me.*

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM,
WITHOUT ETHERIZATION.

Before attempting to determine whether the results in these ether amputations (Table, No. I.) are, or are not favourable to the adoption of Anæsthesia in Surgery, let me in the first place state the results of the previous investigations that have been published by Phillips, Lawrie, and Malgaigne, relating to the mortality of these same amputations, when the same operations were performed without ether. In the year 1837, Mr. Benjamin Phillips brought before the Royal Medico-Chirurgical Society of London, a communication† on the results of amputation of the thigh, leg, and arm, in different countries. From the collection of cases which he laid before the society, Mr. Phillips concluded that the general mortality of these larger amputations amounted to 23 deaths in the 100 operations. The correctness, however, of his conclusions was called in question by the publishing committee of the society, on the idea that the alleged mortality was too great, and he was recommended to investigate the subject more fully before proceeding to publish his observations. Further inquiry served only to satisfy him that his previous results were understated rather than overstated.

Subsequently, in 1844, Mr. Phillips published a table of a still more extensive series of cases.‡ This collection, however, includes the results of private as well as of hospital practice. "They are (says Mr. Phillips) the whole, so far as I know, of the cases of amputation recorded in the periodical literature of this and other countries during the present century. I by no means (Mr. Phillips adds) think that the results furnished by such data will fairly represent the mortality. I believe it will be *understated*, because successful cases are more likely than unsuccessful ones to find their way into print."

The table (No. II. p. 74), extracted from Mr. Phillips's second paper, shows in a summary way the results which he obtained from these sources.

* In No. 49 of the Table, the name of the hospital is not mentioned, as my correspondent unfortunately omitted to date his return. The Paris hospital returns of twenty-two cases (No. 40) are distributed according to the standard of Malgaigne; Dr. Burguières, in a note to me, having stated that he was unable to give the exact number of these amputations which were respectively primary and secondary.

† Observations on the Results of Amputation in different Countries. Medical Gazette, vol. xxii., 1837-38. P. 457.

‡ Medical Gazette, vol. xxxiii., 1843-44. P. 804.

No. I.—TABLE SHOWING, IN DETAIL, THE NUMBER OF INDIVIDUAL AMPUTATIONS AND THEIR RESULTS, IN DIFFERENT HOSPITALS, UPON 302 PATIENTS UNDER ETHERIZATION.

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
			Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.	
			Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary,	Dr. Keith and	—	1	2	1	—	—	5	0	—	—	—	—	—	7	1	
2	Bedford General Infirmary,	Dr. Macintosh,	—	0	3	0	—	—	—	—	—	—	—	—	—	4	0	
3	Birmingham General Infirmary,	Mr. Hurst,	—	0	1	0	2	0	1	0	—	—	—	—	2	4	0	
4	Birmingham Queen's Hospital,	Mr. Amphlett,	—	0	1	0	1	0	3	0	—	—	—	—	1	4	0	
5	Bristol, General Hospital,	Dr. Wright,	—	0	—	—	—	—	3	0	—	—	—	—	—	4	0	
6	Bristol Infirmary,	Dr. Lansdowne and Mr. Mason,	—	0	—	—	—	—	—	—	—	—	—	—	—	6	0	
7	Belfast Hospital,	Mr. Morgan,	1	1	1	1	—	—	—	—	—	—	—	—	—	1	1	
8	Cumberland Infirmary,	Mr. Moore,	1	1	1	1	—	—	1	0	—	—	—	—	1	3	1	
9	Chester Infirmary,	Mr. Page,	—	1	2	1	—	—	2	0	—	—	—	—	1	0	1	
10	Cork South Infirmary,	Mr. Harrison,	—	—	—	—	—	—	—	—	—	—	—	—	1	4	1	
11	Dundee Infirmary,	Dr. Tanner,	—	—	3	0	1	0	2	1	—	—	—	—	2	5	1	
12	Dumfries Infirmary,	Dr. Monro,	1	—	—	—	—	—	1	0	—	—	—	—	1	1	0	
13	Derbyshire General Infirmary,	Mr. Borthwick,	—	1	—	—	1	0	—	—	—	—	—	—	1	0	—	
14	Devon and Exeter Hospital,	Mr. Fox,	—	—	2	0	—	—	1	0	—	—	—	—	1	0	—	
15	Dublin Mercer Hospital,	Mr. James,	—	—	1	1	—	—	—	—	—	—	—	—	3	0	1	
16	Dublin Richmond Surg'l Hosp'l,	Dr. Jamieson,	—	—	3	1	—	—	—	—	—	—	—	—	1	2	1	
17	Edinburgh Infirmary,	Dr. Hamilton,	1	1	2	0	—	—	1	1	—	—	—	—	1	3	1	
18	Edinburgh Infirmary,	Mr. Miller,	2	2	4	1	1	0	3	0	—	—	—	—	3	1	1	
19	Elgin Infirmary,	Dr. Duncan,	—	—	—	—	—	—	—	—	—	—	—	—	7	1	0	
20	Glasgow Hospital,	Dr. Paul,	—	—	8	4	5	1	—	—	—	—	—	—	—	1	0	
21	Hereford Infirmary,	Dr. Orr, and Dr. Steele,	—	—	—	—	—	—	5	2	—	—	—	—	7	17	9	
22	Hull General Infirmary,	Mr. Craven,	1	0	3	0	3	1	—	—	—	—	—	—	—	3	0	
23	Hants, Royal South Infirmary,	Mr. Wandby,	—	—	1	1	—	—	2	0	—	—	—	—	5	1	1	
24	Ipswich Hospital,	Dr. Craven,	—	—	3	0	—	—	2	0	—	—	—	—	1	4	1	
	Carried forward,	Dr. Bullar,	6	5	45	12	14	2	33	4	7	0	12	5	27	3	0	
		Dr. Durrant,	—	—	—	—	—	—	—	—	—	—	—	—	—	3	0	
																	21	

TABLE SHOWING, IN DETAIL, THE NUMBER OF INDIVIDUAL AMPUTATIONS AND THEIR RESULTS, IN DIFFERENT HOSPITALS, UPON 302 PATIENTS UNDER ETHERIZATION, *continued*.

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
			Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Cases.	Deaths.	Cases.	Deaths.
			Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.				
25	Brought forward, .	Mr. Hey, .	6	5	45	12	14	2	33	4	7	0	12	5	27	7	90	21
26	Leeds Infirmary, .	Mr. Curling, .	1	0	4	0	1	0	1	0	—	—	—	—	1	0	1	0
27	London Hospital, .	Mr. Haig, .	2	1	6	3	2	1	4	1	2	1	1	0	4	3	5	0
28	London, St. Bartholomew's Hosp.,	Mr. Liston and	2	1	5	0	—	—	5	1	—	—	1	1	11	2	11	4
29	London, Univ. College Hosp.,	Mr. Cadge, .	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
30	London, King's College Hosp.,	Mr. Ferguson, .	—	—	1	0	—	—	1	0	—	—	—	—	—	—	—	—
31	London, St. George's Hospital, .	Dr. Snow, .	—	—	12	2	1	0	7	0	—	—	1	0	—	—	2	0
32	London, Middlesex Hospital, .	Mr. Shaw, .	1	0	5	0	1	0	1	1	—	—	1	0	1	0	7	1
33	London, Charing-Cross Hospital,	Mr. Avery, .	1	0	2	1	—	—	1	0	—	—	1	0	1	0	3	1
34	London, Westminster Hospital.	Dr. Bird, .	2	0	4	0	—	—	1	0	—	—	1	0	—	—	6	0
35	Leicester Infirmary, .	Mr. Paget, .	—	—	3	2	—	—	4	3	—	—	2	1	2	0	1	0
36	Liverpool Northern Hospital, .	Dr. Bainbrigge, .	—	—	1	0	1	0	—	—	1	0	1	0	—	—	9	6
37	Liverpool Southern Hospital, .	Mr. Morris, .	—	—	2	1	—	—	—	—	—	—	—	—	2	0	2	0
38	Manchester Royal Infirmary, .	Dr. Reid, .	—	—	—	—	2	2	—	1	—	—	—	—	3	2	4	2
39	Newcastle Infirmary, .	Mr. Greenhow, .	—	—	3	0	1	0	1	0	1	0	—	—	2	1	—	—
40	Nottingham General Hospital, .	Mr. Wright, .	3	2	7	2	3	1	5	1	2	1	2	1	8	4	14	5
41	Paris Hospitals, .	Dr. Burquières, .	4	2	1	0	3	1	—	—	—	—	—	—	4	2	1	0
42	Perth Infirmary, .	Dr. M'Farlane, .	1	0	2	0	3	1	1	0	1	0	—	—	5	1	4	0
43	Sussex County Hospital, .	Mr. Parson, .	—	—	2	0	—	—	2	0	—	—	—	—	—	—	4	0
44	Staffordshire, North Infirmary, .	Mr. Turner, .	—	—	3	0	—	—	2	0	—	—	2	0	—	—	7	0
45	Sheffield General Infirmary, .	Mr. Jackson, .	—	—	4	2	—	—	3	0	—	—	—	—	—	—	7	2
46	Salisbury Infirmary, .	Mr. Young, .	—	—	—	—	—	—	2	0	—	—	—	—	—	—	2	0
47	Stockport Infirmary, .	Mr. Rayner, .	—	—	—	—	—	—	—	—	—	—	—	—	—	—	5	0
48	Winchester County Hospital, .	Mr. Wickham, .	1	1	3	0	—	—	—	—	—	—	2	0	—	—	8	0
49	Worcester Infirmary, .	Mr. Sheppard, .	1	0	4	0	1	0	—	—	2	1	—	—	3	2	1	0
	Mr. Stocker, .	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—
	Total,	—	24	12	121	25	32	9	81	13	17	4	27	8	73	25	223	46

No. II.—*Table of the Mortality of 1369 cases of Amputation of the Thigh, Leg, and Arm.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per-centage of Deaths.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Thigh, . .	245	176	72	415	87	21
Leg, . . .	204	88	43	231	61	27
Arm, . .	164	49	29	110	26	24
TOTAL, . .	613	313	51	756	174	23

In the year 1840, Dr. Lawrie, of Glasgow, published an excellent paper* on the results of amputations, with tables showing the rate of mortality from amputation in the Glasgow hospital, from the period of its foundation in 1794 down to 1839. Dr. Lawrie's inquiries yielded an average mortality greater than that of Mr. Phillips, being as high as 36 per cent. The following table, made from data in Mr. Lawrie's paper, contains the results of amputation of the thigh, leg, and arm, in the Glasgow hospital.

No. III.—*Table of the Mortality of 242 Amputations of the Thigh, Leg, and Arm in the Glasgow Hospital from 1794 to 1839.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per-centage of Deaths.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Thigh, . .	35	27	77	92	19	20
Leg, . . .	27	18	66	35	12	34
Arm, . . .	36	18	50	17	3	17
TOTAL, . .	98	63	64	144	34	23

In 1842, a valuable series of papers on the statistics of amputations was published by Professor Malgaigne in the *Archives Générales de Médecine*, his data being derived from the reports of the Parisian hospitals. In these papers, Malgaigne enters largely upon the subject of the mortality of amputations. The following table, compiled from data in

* On the Results of Amputations. Medical Gazette, vol. xxvii. 1841, p. 394.

his returns,* exhibits a mortality still higher than that of the Glasgow hospital.

No. IV.—*Table of the Mortality of 484 Amputations of the Thigh, Leg, and Arm, in the Parisian Hospitals, from 1836 to 1841.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per-centage of Deaths.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Thigh, . .	48	34	70	153	92	60
Leg, . . .	80	51	63	112	55	49
Arm, . . .	30	17	56	61	24	39
TOTAL, . .	158	102	64	326	171	52

These three tables of large collections of cases by Phillips, Lawrie, and Malgaigne, may be properly considered as giving a correct idea of the general mortality of these amputations in hospital practice, and may be used with justice as subjects of comparison with any series of cases similar to them in the whole series of circumstances, except that one whose influence upon the results is to be decided. After, however, I began to collect the results and mortality of the same amputations upon patients in an etherized state from various British and other hospitals, it was objected to the inquiry that it would be unsatisfactory in two respects, viz., that the amputations compared were possibly performed in different classes of hospitals, and at dates so different that I did not consider in my investigation the changes and improvements which might possibly have been introduced into the very methods of operating.

In order, then, at once to enlarge the basis of data for comparison, and to obtain a series of cases still more exactly similar to the collection of ether amputations which I was making, I procured from various British hospitals, through the kindness of different correspondents, and from published data, returns of the latest amputations that had been performed in them immediately previous to the introduction of etherization. These returns are given in detail on page 76. All of the operations have been performed within the eight years, from 1839 to 1846 inclusive. By having this collection of cases as an additional standard, I hoped to avoid all cavil on the ground of any supposed difference in the time, and other collateral circumstances, in which the compared operations were performed.

* Archives Générales de Médecine, vol. lviii. 1842, p. 40.

No. V.—TABLE SHOWING, IN DETAIL, THE RESULTS OF 618 AMPUTATIONS, IN 30 DIFFERENT BRITISH HOSPITALS, IMMEDIATELY BEFORE THE INTRODUCTION OF ETHERIZATION.

No.	Name of Hospital.	Date of the Observations.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
				Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary.	1841-1846	Dr. Macintosh.	5	3	7	2	5	2	15	4	5	0	5	0	15	5	27	6
2	Bristol, St. Peter's Hospital.	1841-1846	Dr. Rogers.	—	—	3	0	—	—	—	—	—	—	—	—	—	—	3	0
3	Bristol Infirmary.	1845-1846	Mr. Morgan.	1	0	17	2	2	0	12	1	3	0	1	0	6	0	30	3
4	Bristol General Hospital.	1846	Mr. Mason.	—	—	2	1	—	—	—	—	—	—	—	—	—	—	2	1
5	Berk's Royal Hospital.	1840-1845	Mr. May.	7	5	5	0	3	0	—	—	1	0	1	0	13	5	6	0
6	Cork Northern Infirmary.	1845-1846	Dr. Bullen.	1	0	2	1	—	—	3	0	—	—	—	—	2	0	5	1
7	Cumberland Infirmary.	1845-1846	Mr. Page.	—	—	4	1	—	—	2	0	2	0	—	—	1	0	6	1
8	Dumfries Infirmary.	1842-1846	Mr. Borthwick.	—	—	6	1	—	—	—	—	1	1	—	—	—	—	1	1
9	Derbyshire General Infirmary.	1845-1846	Mr. Fox.	—	—	5	2	—	—	2	0	—	—	—	—	—	—	11	2
10	Dundee Infirmary.	1844-1846	Dr. Menro.	1	0	2	0	1	1	1	3	—	—	—	—	5	1	9	2
11	Edinburgh Royal Infirmary.	1840-1845	Dr. Peacock.	8	8	35	13	1	4	12	4	3	0	2	1	17	15	49	16
12	Edin Infirmary.	1844-1845	Dr. Paul.	—	—	14	1	1	1	1	0	—	—	—	—	1	1	9	2
13	Glasgow Infirmary.	1840-1844	Dr. King.	11	10	14	6	1	5	6	1	7	4	—	—	32	19	20	7
14	Glasgow Hospital.	1844-1846	Dr. Orr.	10	3	22	8	17	5	13	4	12	3	11	5	39	11	46	17
15	Hull General Infirmary.	1846	Mr. Craven.	—	—	2	0	3	0	—	—	2	0	—	—	5	0	2	0
16	Inverness Infirmary.	1845-1846	Dr. Manford.	1	1	6	0	1	0	—	—	—	—	—	—	2	1	6	0
17	Ipswich Infirmary.	1845-1846	Dr. Durrant.	—	—	—	—	—	—	—	—	—	—	—	—	4	0	—	—
18	London, St. Bartholomew's Hos.	1846	Mr. Haig.	2	1	3	0	1	0	4	1	3	0	3	0	8	1	14	3
19	London University College Hos.	1841-1846	Mr. Cadge.	5	2	16	3	1	1	15	4	1	0	7	4	38	10	—	—
20	Leicester Infirmary.	1845-1846	Mr. Paget.	2	0	3	1	1	0	4	2	2	0	2	0	11	4	14	6
21	Liverpool Northern Hospital.	1845-1896	Dr. Bainbridge.	5	3	8	4	4	1	6	0	3	0	2	0	6	0	9	1
22	Liverpool Southern Hospital.	1846	Mr. Morris.	1	1	3	1	2	1	—	—	—	—	—	—	1	0	1	0
23	Newcastle Infirmary.	1840-1846	Mr. Benson.	7	3	10	2	8	2	10	3	10	2	2	0	25	7	22	5
24	Perth Hospital.	—1847	Dr. McFarlane.	—	—	13	5	—	—	7	0	2	1	2	1	—	—	22	8
25	Sussex County Hospital.	1844-1846	Mr. Parson.	2	2	2	0	2	1	3	0	3	1	1	0	7	4	6	2
26	Salisbury Infirmary.	1845-1846	Mr. Young.	2	0	3	0	3	2	3	1	1	0	1	0	6	2	7	1
27	Stockport Infirmary.	1845-1846	Mr. Rayner.	2	2	2	0	—	—	1	0	2	0	—	—	4	2	3	0
28	Sheffield Infirmary.	1845-1846	Mr. Jackson.	—	—	1	0	3	0	—	—	—	—	—	—	3	0	1	0
29	Forester Infirmary.	1845-1846	Mr. Shepherd.	—	—	5	0	1	0	—	—	4	2	—	—	5	2	9	0
30	York County Hospital.	1845-1846	Mr. Hey.	—	—	1	0	1	1	3	0	2	1	1	1	3	2	5	1
Total.				73	45	211	62	80	26	135	23	77	17	42	10	230	88	388	95

The data in the preceding table, No. V. (p. 76), when condensed into the tabular form, afford the results in the following table, No. VI.

No. VI.—*Table of the Mortality of 618 Amputations of the Thigh, Leg, and Arm, without Etherization, performed during the last few years in 30 British Hospitals.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per-centage of Deaths.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Thigh. . .	73	45	63	211	62	29
Leg. . . .	80	26	32	135	23	17
Arm. . . .	77	17	22	42	10	24
TOTAL, . .	230	88	38	388	95	24

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM UPON PATIENTS IN AN ETHERIZED STATE.

In the preceding lengthened Table, No. I. (p. 72,) I have given from forty-nine different hospitals the detailed reports of 302 amputations of the thigh, leg, and arm. When these 302 amputations are reduced into a tabular form, similar to those which I have used for stating the data of similar amputations without ether, they present the following results:—

No. VII.—*Table of the Mortality of 302 Amputations of the Thigh, Leg, and Arm, under Etherization.*

Seat of the Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per-centage of Deaths.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Thigh, . .	24	12	50	121	25	20
Leg, . . .	32	9	28	81	13	16
Arm, . . .	17	4	23	27	8	29
TOTAL, . .	73	25	34	229	46	20

I shall now proceed to contrast these results with the results of

the same operations in the same class of hospitals, and when performed upon patients not in an etherized state.

Before doing so, however, let me observe in passing, that the data I have adduced in Tables No. I. and V., (pp. 72, 76) have been objected to on the ground that they are collected from too many different hospitals, and too many different sources. But, on the contrary, I believe all our highest statistical authorities will hold that this very circumstance renders them more, instead of less trustworthy. Professor Chomel of Paris, after pointing out the first requisite for a successful statistical comparison of therapeutic or other results—viz., a sufficient similarity between the number of collated cases—adds, as the second condition, “that the data be numerous, collected at different times, in different places, and, if possible, by several observers. It is easily seen (he adds) that the results of a number of facts too limited, collected in a short space of time, in a single place, and by a single observer, however exact as regards that individual series of data, may yet be very different from, or even the reverse of conclusions drawn from a larger series, and one collected under various circumstances.”*

COMPARISON OF THE MORTALITY FOLLOWING THE LARGER AMPUTATIONS OF THE LIMBS, 1. WITHOUT, AND 2. WITH ETHERIZATION.

The major amputations of the limbs, including those of the thigh, leg, and arm, are generally fatal in hospital practice in the proportion of about 1 in every 2 or 3 operated upon. In the Parisian hospitals, the fatality, according to Malgaigne, amounts to upwards of 1 in 2. In Glasgow, it is $2\frac{1}{2}$. In British hospitals, I found that under these amputations 1 in $3\frac{1}{2}$ died. The same operations, performed in the same hospitals, and upon the same class of patients, in an anæsthetic state, present a mortality of 23 in 100, or 1 in 4, only. The following table shows the amount of the individual cases, and the per-centage of deaths in different collections, with the corresponding proportion of deaths in those operated on in an etherized state.

No. VIII.—*Table of the Mortality of Amputation of the Thigh, Leg, and Arm.*

Reporter.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Parisian Hospitals—Malgaigne,	484	273	57 in 100
Glasgow Hospital—Lawrie,	242	97	40 in 100
General Collection—Phillips,	1369	487	35 in 100
British Hospitals—Simpson,	618	183	29 in 100
Upon Patients in an Etherized State,	302	71	23 in 100

* Bulletin de l'Acad. Roy. de Médecine. Seance du Mai 2, 1837.

The evidence which the preceding table affords in favour of the greater safety of amputation with ether than without it, is sufficiently strong and striking. While 23 in 100 died under the amputations named, when the operations were performed upon patients in an anæsthetic state; 29 in every 100 died under the same amputations in the same hospitals when the patients were not etherized;—in the Glasgow hospital as many as 40 in 100 died; and in Paris, as many as 57 per cent. In other words, out of every 100 persons submitted to amputations of the thigh, leg, or arm, the lives of 6 were, by the employment of etherization, saved above the average number of the same operations in British hospitals;—17 lives in each 100 were saved, if we take the Glasgow returns as a standard of comparison; the average mortality was, under ether, less by 34 in every 100 cases than that which was found by Malignaigne to accompany the same operations in the Parisian hospitals.

But probably, to most minds, this comparison would be rendered more clear and simple, if we took not a class of operations, but a single operation as a standard and medium of comparison. For this purpose, let us select amputation of the thigh as the *individual* operation regarding which we possess the largest series of observations.*

COMPARISON OF THE MORTALITY FOLLOWING AMPUTATIONS OF THE THIGH, 1. WITHOUT, AND 2. WITH ETHERIZATION.

There are few or none of the operations deemed justifiable in surgery, that are more fearfully fatal in their results than amputation of the thigh. "The stern evidence (says Mr. Syme) of hospital statistics shows, that the average frequency of death is not less than from 60 to 70 per cent;"+ or above 1 in every 2 operated on die. Out of 987 cases of amputation

* One objection may be urged against the comparison of the results of a single operation, with or without etherization, that I am now about to institute, on the ground, viz.,—that the number of cases (145) is too limited to afford a result that is perfectly decisive. I am perfectly willing to admit the justness of this remark in a statistical point of view, and to hold this part (and indeed the whole of the present inquiry) as, so far, the commencement and nucleus merely of a more full and lengthened investigation by other hands. At the same time, I have, during the course of the inquiry, had the conviction impressed upon me, that future results will more and more confirm those that I have here stated in the text, and be still more in favour of etherization; for no small number of the operations reported to me were, in the first periods of the new practice, doubtlessly performed upon patients in whom the anæsthesia was by no means entire and complete, in consequence of imperfection in the forms of apparatus, in their management, in the dose given, &c.; and, I believe, that as the profession becomes more accomplished and certain in the use of such measures, the resulting effects will become proportionally happier and more favourable.

† Monthly Journal for May 1845, p. 337.

of the thigh collated by Mr. Phillips, 435 proved fatal; or 44 in every 100 were lost.* "On referring (observes Mr. Curling) to a table of amputations in the hospitals of London, performed from 1837 to 1843, collected with care by a private society to which I have the honour of belonging (the Medical Society of Observations), I find 134 cases of amputation of the thigh and leg, of which 55 were fatal, giving a mortality of 41 per cent."† Out of 201 amputations of the thigh performed in the Parisian hospitals, and reported by Malgaigne, 126 ended fatally. In the Edinburgh Infirmary 21 died out of 43. Dr. Lawrie found the mortality attendant upon this operation in the Glasgow hospital to amount to 46 deaths in 127 cases. In the collection of cases from 30 different British hospitals which I have published in table No. V.(p. 76), 284 cases of amputation of the thigh are reported; 107 out of these 284 operations proved fatal. On the contrary, I have collated 145 cases in which the same operation has been performed during the past year in British hospitals upon patients in an etherized state. Out of these 145 cases of amputation of the thigh, only 37 proved fatal. Or, in other words, the fatality was not greater than 1 in every 4 operated on when the patients were previously etherized. It was as high as 1 in every 2 or 3 operated upon when the patients were not previously etherized. The following table presents these results in a more clear form :—

No. IX.—*Table of the Mortality of Amputation of the Thigh.*

Name of Reporter.	No. of Cases.	No. of Deaths.	Per-centage of Deaths.
Parisian Hospitals—Malgaigne,	201	126	62 in 100
Edinburgh Hospital—Peacock,	43	21	49 in 100
General Collection—Phillips,	987	435	44 in 100
Glasgow Hospital—Lawrie,	127	46	36 in 100
British Hospitals—Simpson,	284	107	38 in 100
<i>Upon Patients in an Etherized State, . . .</i>	<i>145</i>	<i>37</i>	<i>25 in 100</i>

The preceding figures speak in a language much more emphatic than any mere words that I could employ in favour of anæsthesia, not only as a means of preserving surgical patients from pain, but as a means also of preserving them from death. Between even the lowest mortality in the table without ether, 36 in 100, and the rate of mortality with it, 25 in 100, there is the difference of 11 per cent. That is to say, according to this standard, out of every 100 patients submitted to amputation of

* Medical Gazette for 1844, p. 805.

† Address to the Hunterian Society of London, 1848, p. 31.

the thigh without anæsthesia, 11 more would die from the operation than if the same 100 patients were submitted to the same operation in a state of anæsthesia. And if the condition of anæsthesia effects thus a saving of 11 lives in every 100 amputations of the thigh;—then out of every 1000 such operations the lives of 110 patients would be preserved by the use of antipathic means.

If we compare these results with the standard of Mr. Phillips, the contrast is still more startling. Out of 987 amputations of the thigh collected by him, 435 proved fatal; or 44 in the 100. Out of 145 amputations of the thigh under anæsthesia, 37 proved fatal, or 25 in 100. According to this comparison, the amount of persons saved from death in amputation of the thigh by the patients being rendered anæsthetic during the operation, amounts to 19 lives in every 100 operations performed.

In conclusion, let me add, that when etherization first began to be employed in surgical operations, it was eagerly argued that its adoption produced a greater tendency to primary and secondary hemorrhage, to imperfect union of the wounds, to pneumonia, &c. If my space had permitted, it was my intention to show, from the analyses of the three hundred cases of amputation reported to me, that these various allegations were foundationless and imaginary,*—that such consequences were not so frequent after amputations with ether as after amputations previously performed without it,—that as the casualties were reduced in number, so were also the attendant accidents and complications.† But I believe such proof to be at the present day superfluous, as few or none now maintain such opinions. When writing to me as early as in June last on this subject, the late lamented Mr. Liston stated what all the subsequent experience of our ablest surgeons here and in London has confirmed. “The ether (says he) produces no bad effect, as far as I can see. There is no change in the blood, nor in the vessels, or muscles. The recoveries are, at least, quite as good as before it was employed.” An excellent surgical pathologist (Mr. Curling, surgeon to the London Hospital)‡ has more recently afforded still stronger testimony to the same effect. “I have carefully watched (says he) the progress of cases, after operations of various kinds performed upon patients in a state of anæsthesia, and I can with confidence declare

* Some of my correspondents, who expressed the strongest opinions in regard to the reality of these supposed evil consequences, have, I know, now abandoned such opinions as utterly untenable.

† In my communication to the Medico-Chirurgical Society, in June last, I went over this ground at some length.

‡ Address to the Hunterian Society of London, 1848, p. 23.

that, so far as my present experience has reached, the constitutional symptoms have been milder, and the cases have proceeded more satisfactorily, than after operations in which no means had been taken to prevent pain. Several of my surgical friends can fully confirm this statement."

I have also avoided entering into the theoretical question,—How does anæsthesia render severe operations less fatal and dangerous in their consequences? I have already shown (*Journal* for September, p. 164) that the endurance of severe pain is in itself depressing and destructive; and apparently the anæsthetic state saves the patient from this suffering and its effects; as well as saves him, in some degree, from the shock of the operation and its consequences. When writing, in 1839, on the subjects of pain and shock, and on certain states connected with or produced by wounds or injuries, Professor Burns of Glasgow offered some remarks bearing directly on the present subject, and which are more valuable as they were written without any theory, and without any prospect of such a state as he speaks of being capable of being artificially induced. I shall quote them in his own words:—"The mere lopping off of the member, by the immediate abridgement of the quantity of living body, the instant loss of so large a portion, which was formerly acting along with the system, is productive of serious evil to it, from the sympathy which universally prevails. But if the *nervous system become in part torpid, so as to prevent this sympathy*, or to be incapable of maintaining it, the loss of a member, or what is, in one respect, the same, the loss of its connexion with the system, and its failure in power, and action, and sensibility, may not have the same bad effect."—*Principles of Surgery*, Vol. I. p. 493.

PART II.

ANÆSTHESIA IN MIDWIFERY.

* * "Not poppy, nor mandragora,
Nor all the drowsy syrups of the world,
Shall ever medicine thee to such sweet sleep."
SHAKESPEARE.

CHAPTER I.

ON THE INHALATION OF SULPHURIC ETHER IN THE PRACTICE OF MIDWIFERY.

ABUNDANT evidence has of late been adduced, and is daily accumulating, in proof of the inhalation of sulphuric ether being capable, in the generality of individuals, of producing a more or less perfect degree of insensibility to the pains of the most severe surgical operations. But whilst this agent has been used extensively, and by numerous hands, in the practice of surgery, I am not aware that any one has hitherto ventured to test its applicability to the practice of midwifery. I am induced, therefore, to hope that the few following hurried and imperfect notes, relative to its employment in obstetric cases, may not at the present time prove uninteresting to the profession.

Within the last month I have had opportunities of using the inhalation of ether in the operation of turning, in cases of the employment of the long and of the short forceps, as well as in several instances in which the labour was of a natural type, and consequently required no special form of artificial aid.

The first case in which I employed the ether vapour, occurred on the 19th of January. Some details of the result have been already published in the February number of the *Edinburgh Monthly Journal of Medical Science* (see p. 639). The pelvis of the mother was greatly contracted in its conjugate diameter from the projection forwards and downwards of the promontory of the sacrum; the lumbar portion of the spine was distorted; and she walked very lamely. The

present was her second confinement. Her first labour had been long and difficult; she began to suffer on a Monday, and after a protracted trial of the long forceps, was at last delivered by craniotomy late on the subsequent Thursday night. Even after the cranium had been fully broken down, a considerable time and much traction had been required to drag the diminished and mutilated head of the infant through the contracted brim of the pelvis; and she was long in recovering. Contrary to the urgent advice of her medical attendant, Mr. Figg, he was not made aware of her present or second pregnancy till she had arrived at nearly the end of the ninth month. It was thus too late to have recourse to the induction of premature labour, which had been strongly pressed upon her as the only means of saving her child, should she again fall in the family way. The pains of her second labour commenced in the forenoon of the 19th. I saw her with Mr. Figg at five o'clock in the afternoon, and again at seven. The os uteri was pretty well dilated, the liquor amnii not evacuated, the presenting head very high, mobile, and difficult to touch; and a pulsating loop of the umbilical cord was felt floating below it in the unruptured bag of membranes. From five to nine o'clock the pains seemed only to push the circle of the os uteri further downwards, without increasing its dilatation, or making the head in any degree enter into the pelvic brim. Assisted by Dr. Zeigler, Dr. Keith, and Mr. Figg, I shortly after nine o'clock made the patient inhale the ether vapour. As she afterwards informed us, she almost immediately came under the anodyne influence of the ether. But in consequence of doubts upon this point, its use was continued for nearly twenty minutes before I proceeded to turn the infant (as I had previously predetermined to do). A knee was easily seized, and the child's extremities and trunk readily drawn down; but extreme exertion was required in order to extract the head. At length it passed the contracted brim with the anterior part of its right parietal bone deeply indented by pressure against the projecting promontory of the sacrum, and the whole cranium flattened and compressed laterally. The infant gasped several times, but full respiration could not be established. The transverse or biparietal measurement of its head, at the site of the indentation, was, in its compressed state, not more than $2\frac{1}{2}$ inches. Hence we judged the conjugate diameter of the pelvic brim not to exceed this. The infant was large, and rather above the usual size. It weighed 8 lbs. On afterwards examining the head and removing the scalp, no fracture could be found at the seat of the indentation. The thin parietal bone had merely bent inwards.

On questioning the patient after her delivery, she declared

that she was quite unconscious of pain during the whole period of the turning and extracting of the infant, or indeed from the first minute or two after she first commenced to breathe the ether. The inhalation was discontinued towards the latter part of the operation, and her first recollections on awaking were "hearing," but not "feeling," the head of the infant "jerk" from her (to use her own expressions), and subsequently she became more roused by the noise caused by the preparation of a bath for the child. She quickly regained full consciousness, and talked with gratitude and wonderment of her delivery, and her insensibility to the pains of it. Next day I found her very well in all respects. I looked in upon her on the 24th (the fifth day after delivery), and was astonished to find her up and dressed, and she informed me that on the previous day she had walked out of her room to visit her mother. Mr. Figg informs me that her further convalescence has been uninterruptedly good and rapid.

I have previously alluded to two cases of delivery by the forceps, in which the patients were under the action of ether at the time of the operation. The woman in the first of these cases was brought into the Royal Maternity Hospital, in strong labour, early on the morning of the 3d February. It was her second confinement. At her first accouchement (seven years before), she had been delivered by instruments, in Ireland, and had been informed by the attendant practitioner, that artificial delivery would be similarly required at her future labours. I saw her between ten and eleven o'clock, A.M. The os uteri was well dilated, the membranes ruptured, and the pains extremely strong and frequent; but the large head of the child seemed not to enter fully into the brim, and was little affected by the powerful uterine contractions under which the patient was suffering. By three o'clock her pulse had risen to above 125 beats a minute, and it appeared to the medical officers present, that it would be improper to allow the ineffectual and exhausting efforts of the patient to be longer continued. She was then, at my request, brought under the influence of ether. Dr. Moir, with great skill, applied the long forceps upon the head of the child. He subsequently was obliged to use strong traction during the pains that followed, and becoming temporarily fatigued with his efforts, I supplied his place. After the head fully passed the brim, the forceps were laid aside, and one or two uterine contractions finished the delivery. The child was large and strong, and cried vigorously soon after it was expelled. During the whole of this severe operation the patient appeared quiet and passive. The cries of her child speedily roused her from her etherized state, and she subsequently assured Dr. Moir

that she had felt comparatively little or no pain during the whole operation and delivery.

On the evening of the 12th February, I saw another forceps case with my friend, Dr. Graham Weir. The patient was advanced in life, and it was her first confinement. The waters had escaped early, and the anterior lip of the uterus had subsequently become forced down in a very swelled and œdematous state before the head of the infant. After this obstruction was overcome, the child's head speedily descended upon the floor of the pelvis; but it was there impeded in its further progress by the narrow transverse diameter of the outlet. Under the compression of the converging tuberosities of the ischia, the bones of the foetal cranium soon began to overlap; but at last, no further progress being made, the patient becoming exhausted by a continuous labour of about twenty-four hours, and the soft parts being evidently well relaxed and prepared, Dr. Weir applied the short forceps, and extracted a living infant. For a considerable time before this operation was adopted, I exhibited the vapour of ether to the patient; under it she speedily became quite narcotized. Its action was kept up, and the pains appeared to be so strong as almost to warrant the idea that nature would yet be sufficient; but ultimately, instrumental delivery was, as I have already stated, had recourse to. The mother did not fully recover from her state of etherization for ten or fifteen minutes after delivery, and then stated that she was quite unaware of anything that had been done, and of what had occurred. Dr. Weir informs me that this patient was up on the fourth day after delivery, and felt by that time so perfectly well, that she was with difficulty persuaded by the nurse to abstain from walking about the house as usual.

As far as they go, the preceding cases point out one important result:—in all of them, the uterine contractions continued as regular in their recurrence and duration after the state of etherization had been induced, as before the inhalation was begun. The emotion of fear has appeared to me to suspend, in one or two nervous patients, the recurrence of the first pains, after the apparatus was adjusted and its employment commenced; but this effect speedily passed off; and as yet I have seen no instance in which the pains were sensibly diminished in intensity or frequency after the ether had fairly begun to act. Indeed, in some cases they have appeared to me to have become increased as the consciousness of the patient became diminished. This has more particularly occurred with one or two patients, who breathed ether, combined with tincture of ergot, or containing a solution of its oil. A woman was brought into the Maternity Hospital on the 28th January, after being in

labour for 30 or 40 hours. It was her second child. Subsequently to her entering the hospital, at seven P.M., scarcely any decided uterine contraction could be said to take place. The os uteri was well opened, but the head was still high in the pelvis; and when I saw her at four A.M. of the following morning, nine hours after her entrance into the hospital, little or no advance whatever had been made, and the case was becoming an anxious one. She was then made to inhale equal parts of sulphuric ether and tincture of ergot. In the course of a few minutes a series of extremely powerful uterine contractions supervened, and the child was born within a quarter of an hour of the commencement of inhalation. The mother subsequently declared that she recollected nothing at all of her delivery, except the removal of the after-birth. In this case, was the re-excitement of strong pains the result of the action of the sulphuric ether, or of the ergot, or of both? Or was it a simple but very strange coincidence? More facts than I yet possess are necessary to decide such a question; but I have seen some cases which lead me to believe that other therapeutic agents besides those I have named may be readily introduced into the system by means of pulmonary inhalation.*

A more extensive and careful series of investigations than I have yet been able to institute, may perhaps show that in some constitutions, and under some circumstances or *degrees* of intensity, the process of etherization may possibly interfere with the uterine contractility, particularly in the earlier stages of the labour. At the same time, various analogies would lead us to expect that, as I have hitherto found, the action of the uterus would go on uninterruptedly, when the psychical influence of the mind and purely cerebral functions were suspended, as in the more complete states of etherization. At all events, if we

* Dr. Richard Pearson, who, in 1795, was, I believe, the first person that recommended the inhalation of sulphuric ether as a therapeutic agent (see his *Account of the Nature and Properties of different kinds of Airs*, p. 24), suggested also the use of it impregnated with opium, squill, cicuta, &c.; and he speaks of the effect of "an emetic given in this manner." He employed the simple sulphuric ether vapour in some cases of phthisis, asthma, hooping cough, croup, and catarrh, recommending it to be inhaled (after being rectified and washed), from a cup—through an inverted funnel—or, with children, by "wetting a handkerchief with it, and holding it near the nose and mouth." See *Medical Facts and Observations*, for 1797, vol. vii. p. 96. In the 13th volume of the *Dictionnaire des Sciences Medicales* (1816) p. 385, Nysten has described a particular apparatus, like some of our modern forms, for the inhalation of sulphuric ether. See also vol. xvii. p. 134.—Vaporizable substances, when introduced into the system in this manner, probably pass undigested and unchanged into the circulation, and "seem (observes Wagner) to make their way into the blood through the unbroken vascular membrane [of the bronchial cells] with the *same* certainty and ease as when they are injected *directly* into the veins," (*Elements of Physiology*, 1842, p. 443.) Will this not explain both the rapidity and intensity of their actions when thus used?

may judge from the analogous experiments of Vollkmann, Bidder, and Kölliker, upon the simple contractions and rhythmic reflex actions of the heart, intestines, &c., the motory nervous powers of the uterus belong to the ganglionic and to the spinal systems, and are not in any necessary dependence upon the brain or mind. Indeed, Ollivier and Nasse have published cases of perfect paraplegia, notwithstanding which the act of parturition in the human female proceeded regularly in its course, and without conscious pain. In the one case (Ollivier's), the cord was compressed and destroyed from the first to the fourth dorsal vertebra by a collection of acephalocysts;* and, in the other instance (Nasse's), complete paralysis had followed a fracture of the third and fourth cervical vertebræ.† Of course such lesions necessarily prevented the brain exerting any influence upon the uterus, or its contractions.

Long ago, in discussing this subject, Haller adduced the authority of Harvey, Smellie, Lamotte, &c., to prove that uterine contractions and labour may go on with the mother, "*ignara, stupida. et sopita, et immobili, et apoplectica, et epileptica, et convulsionibus agitata,† et ad summum debili.*"§ Deneux mentions a fact still more in point, because in it the analogy with the operation of ether is still stronger, or indeed identical. "A woman," says he,|| "was brought to the Hotel Dieu at Amiens in a comatose state, in consequence of her taking spirituous liquors since the commencement of labour. She was delivered in the natural manner in this state; the sleep continued for some time after delivery. The woman, on awaking, much surprised at finding her delivery completed, congratulated herself on having made so happy a discovery, and declared she would make use of it if she had again occasion."¶

* *Traité de la Moelle Epiniere*, p. 784.

† *Untersuchungen zur Physiologie, &c.* Dr. Cheyne reports a case of fatal hemorrhagic apoplexy and hemiplegia, in which, without any apparent pains, "the uterus (observes Dr. Kellie) appears, as an involuntary muscle, to have acted in the most perfect manner in expelling the fœtus and secundines," the day before death. The child was born alive. *Cases of Apoplexy and Lethargy*, pages 91 and 161.

‡ "During the continuance of puerperal convulsions, uterine action is *not* suspended, although no signs of pain are manifested by the woman, if she remain comatose." Dr. F. Ramsbotham's *Obstetric Medicine* (1844) p. 455.

§ *Elementa Physiologiæ*, tom. viii. p. 420.

|| *Recueil Periodique de la Société de Médecine*, April, 1818.

¶ The celebrated case of the Countess de St. Geran is sufficiently remarkable in relation to the present subject. See full and long details of it in Gayot's *Causes Celebres*, tom. i. p. 142 to 266. After the Countess had been nine hours in labour with her first child, the midwife in attendance exhibited to her a potion (*breuvage*), which rendered her insensible till the following morning. When the Countess then awoke to consciousness, she found herself bathed in blood, the abdominal tumour fallen, and all the signs of recent delivery present; but the child born during her

In obstetric, as in surgical practice, the degree of insensibility produced by etherization, and its accompanying phenomena, differ much in different instances. In some, a state of total apathy and insensibility seems to be produced; others move about and complain more or less loudly during the uterine contractions, though afterwards, when restored to their state of common consciousness, they have no recollection of any suffering whatever, or, indeed, of anything that had occurred during the inhalation and action of the ether; others, again, remain quite aware and conscious of what is going on around them, and watch the recurrence of the uterine contractions, but feel indifferent to their effects, and not in any degree distressed by their presence; and in another class again, the attendant suffering is merely more or less diminished and obtunded, without being perfectly cancelled and annulled.

On the evening of the 13th inst., in two cases that rapidly followed each other, I witnessed, in the above respect, two very different conditions induced by the use of the ether. The patients (who each had borne several children previously) were both placed under the influence of it just as the os uteri became fully opened, and in neither did the full expulsion of the infant through the pelvic passages require above twelve or fifteen minutes. My first patient (the wife of a clergyman) subsequently stated, that she knew all that was said and done about her, was aware of the pains being present, but felt no distress from any of them till the supervention of the last strong contraction which drove the head out of the vulva, and the feeling then seemed to partake of the character of strong pressure, rather than of actual pain. Subsequently she told me, she could only look back with regret to the apparently unnecessary suffering she had endured in the birth of her former infants. The second patient, a lady of a timid temperament, and very apprehensive about the result of her present confinement, was induced with difficulty to inhale the ether vapour; but it speedily affected her when once she did begin. In two or three minutes she pushed the apparatus from her mouth, talked excitedly to a female relative present, but was immediately induced to recommence the inhalation; and subsequently, according to her own statement, "wakened out of a dream,

state of insensibility had been removed, and its existence was even denied to her. It was years afterwards proved, to the satisfaction of the French law courts, that the Countess had been delivered of a male child, during an induced lethargic condition, and that the infant had been surreptitiously conveyed away to a distance, and brought up as the son of a poor man. The child's claims were, after much litigation, fully acknowledged; he was restored to his parents, and ultimately succeeded to his father's title. What *Nepenthean "brevage"* could possibly produce the alleged effect?

and unexpectedly found her child born." Like many others, she thought hours instead of minutes had elapsed, from the commencement of the inhalation to the period of the complete restoration of consciousness. Making apparently an effort of memory, she afterwards inquired if she had not once awakened out of her dreamy state, and spoken some nonsense to her friend.

A careful collection of cautious and accurate observations will no doubt be required, before the inhalation of sulphuric ether is adopted to any great extent in the practice of midwifery. It will be necessary to ascertain its precise effects, both upon the action of the uterus, and of the assistant abdominal muscles; its influence, if any, upon the child; whether it gives a tendency to hemorrhage or other complications; the contra-indications peculiar to its use; the most certain modes of exhibiting it; the length of time it may be employed, &c.* In no case have I observed any harm whatever to either mother or infant, follow upon its employment. And, on the other hand, I have the strongest assurance and conviction, that I have already seen no small amount of maternal suffering and agony saved by its application. The cases I have detailed sufficiently show its value and safety in cases of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtlessly be. But in modifying and obliterating the state of conscious pain, the nervous *shock*† otherwise liable to be produced by such pain,—particularly whenever it is extreme, and intensely waited for and endured,—is saved to the constitution, and thus an escape gained from many evil consequences that are too apt to follow in its train.‡ Granting that experience will yet be able

* I have, during labour, kept patients under its influence for upwards of half an hour. In exhibiting it, the first, or exhilarating stage of its effects should be passed through as rapidly as possible, and the patient never allowed to be excited or irritated by the nurse or others. I have heard its use strenuously denounced on the ground that its effects, though good and evanescent, are still of an intoxicating character. But on the same ground, the use of opium, &c., &c., in medicine, to relieve pain and procure sleep, should be equally reprobated and discarded.

† On the extent of the nervous *shock* accompanying human parturition, see Dr. Hamilton's Practical Observations, p. 179, &c.; and Dr. Churchill's Chapter on Convalescence after Labour, in his work on the Diseases of Pregnancy and Child-bed, p. 240, &c.

‡ On what division or divisions of the nervous system does the nervous *shock* operate—the cerebral, spinal, or ganglionic? If on the former, it should be kept in abeyance by due etherization. Some years ago I saw Dr. J. Argyll Robertson, when he was Acting Surgeon at the Royal Infirmary, amputate, at the shoulder-joint, an arm sadly shattered an hour or so before by a railway injury. The man at the time of receiving the injury, during the operation, and for several hours afterwards, was in a state of insensibility from deep intoxication; and at last wakened up, not knowing what had happened. His recovery was rapid and un-

to prove its safety and efficacy in modifying and annulling the pains of labour, will (I have repeatedly heard the question asked) the state of etherization ever come to be generally employed with the simple object of assuaging the pains of natural parturition? Or (as the problem has not unfrequently been put to me) would we be "justified" in using it for such a purpose? In conclusion, let us consider this point for a moment.

Custom and prejudice, and, perhaps, the idea of its inevitable necessity, make both the profession and our patients look upon the amount and intensity of pain encountered in common cases of natural labour, as far less worthy of consideration than in reality it is. Viewed apart, and in an isolated light, the degree of actual pain usually endured during common labour is as great, if not greater, than that attendant upon most surgical operations. I allude particularly to the excessive pain and anguish, which in nine out of ten cases accompany the passage of the child's head through the outlet of the pelvis and external parts. Speaking of common or natural labour in its last stages, Dr. Merriman observes, the pulse gradually "increases in quickness and force; the skin grows hot; the face becomes intensely red; drops of sweat stand upon the forehead; and a perspiration, sometimes profuse, breaks out all over the body; frequently violent tremblings accompany the last pain, and at the moment that the head passes into the world, the extremity of suffering seems to be beyond endurance."* Or, take the picture of the suffering of the mother in the last stage of natural labour, as portrayed by the most faithful of living observers—Professor Nægele of Heidelberg—"The pains (he observes) of this stage are still more severe, painful, and enduring; return after a short interval, and take a far greater effect upon the patient, than those of the previous stage. Their severity increases so much the more from the additional suffering arising from the continually increasing distension of the external parts. They convulse the whole frame, and have hence been called the *dolores conquassantes*. The bearing down becomes more continued, and there is not unfrequently vomiting. The patient quivers and trembles all over. Her face is flushed, and with the rest of the body, is bathed in perspiration. Her looks are staring and wild; her features alter so much that they can scarcely be recognised. Her impatience rises to its maximum with loud crying and wailing, and frequently expressions

interrupted. Would it have been so if his nervous system had been sufficiently alive to the double shock of the operation and injury? Out of eighteen cases of primary amputation, performed during four years in the Edinburgh Hospital, and mentioned in Dr. Peacock's Report of the Institution (1843), this man and another patient were the only two out of the eighteen that survived.

* Synopsis of Parturition, p. 15.

which, even with sensible, high-principled women, border close upon insanity. Everything denotes the violent manner in which both body and mind are affected.”*

I have stated that the question which I have been repeatedly asked is this—will we ever be “justified” in using the vapour of ether to assuage the pains of natural labour? Now, if experience betimes goes fully to prove to us the safety with which ether may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured (as shown in the descriptions of Merriman, Nægele, and others),† I believe that the question will require to be quite changed in its character. For, instead of determining in relation to it whether we shall be “justified” in using this agent under the circumstances named, it will become, on the other hand, necessary to determine whether on any grounds, moral or medical, a professional man could deem himself “justified” in withholding, and *not* using any such safe means (as we at present presuppose this to be), provided he had the power by it of assuaging the pangs and anguish of the last stage of natural labour, and thus counteracting what Velpeau describes as “those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,”‡ which accompany the termination of natural parturition in the human mother.

EDINBURGH, FEBRUARY 18, 1847.

* *Lehrbuch der Geburtshülfe*, p. 104. See *British and Foreign Medical Review*, vol. xix. p. 64.

† Dr. Rigby in his *System of Midwifery*, p. 103, observes, “This is the moment of greatest pain, and the patient is quite wild and frantic with suffering; it approaches to a species of insanity,” &c., &c.

‡ *Traité des Accouchemens*, vol. i. p. 449. “Ces cris perçans, cette agitation si vive, ces efforts excessifs, ces angoisses inexprimables, ces douleurs qui paraissent intolérables,” &c.

CHAPTER II.

SUPERINDUCTION OF ANÆSTHESIA IN NATURAL AND MORBID PARTURITION: WITH CASES ILLUSTRATIVE OF THE USE AND EFFECTS OF CHLOROFORM IN OBSTETRIC PRACTICE.*

Serve me—as Mandragora—that I may sleep.
WEBSTER'S DUCHESS OF MALFY.

But there is
No danger in what show of sleep it makes,
More than the locking up the spirits a time,
To be more fresh, reviving.
SHAKESPEARE'S CYMBELINE.

AMONG the many improvements by which the operative part of medicine has, from time to time, been enriched, few or none have exerted a more potent, or a more beneficial influence over its advancement and progress than the introduction, in the 16th century, of the application of ligatures to arteries, with the object of arresting the hemorrhage attendant upon surgical wounds and operations. Previously to that time, surgeons had no other means of stemming the flow of blood—after amputation of the limbs for instance—than by scorching over the raw and bleeding wound with a red-hot iron, or by plunging it into boiling pitch, or by applying strong potential cauteries to its surface. With laudable efforts to diminish the fearful severities of their practice, they exerted their ingenuity in devising, as it were, refinements upon these necessitous cruelties. Thus Hil-danus, the patriarch of German surgery, amputated the limbs of his patients with red-hot knives, in order that he might divide the flesh and scar up the vessels at one and the same time. Upon all these practices, the great and happy suggestion of Ambrose Paré, viz., to shut up the bleeding vessels, by constricting or tying them with slender ligatures, was a vast and mighty improvement. It at once made the arrestment of hemorrhage in operations far more simple, more certain, and more secure. It saved immeasurably the sufferings of the patients, while it added immeasurably to their safety. But the practice was new, and an innovation; and consequently, like all other innovations in medical practice, it was, at first and for long, bitterly decried and denounced. The College of Phy-

* Read to the Medico-Chirurgical Society of Edinburgh, at their meeting on the 1st December, 1847.

sicians of Paris attacked Paré for his proposed new practice: they attempted, by the authority of the French Parliament, to suppress the publication and dissemination of his observations; and, for nearly a long century afterwards, some of the Hospital surgeons of Paris continued, with the characteristic obstinacy of the profession, to prefer cauterizing bleeding arteries "with *all* the ancients," rather than simply tie them "after the manner of a few ignorant and presumptuous moderns."* "Without" (writes the late Mr. John Bell)—"without reading the books of these old surgeons, it is not possible to imagine the horrors of the cautery, nor how much reason Paré had for upbraiding the surgeons of his own time with their cruelties. . . . The horrors of the patient, and his ungovernable cries, the hurry of the operators and assistants, the sparkling of the (heated) irons, and the hissing of the blood against them, *must* have made terrible scenes; and surgery *must*, in those days, have been a horrible trade."†

The sentiments which Mr. Bell here expresses are those with which the human mind often *looks back* upon our opinions and practices, when these opinions and practices are past and gone, and have become mere matters of history. In the above, as in many other instances, we never become fully awakened to the cruelty and enormity of some of our established doctrines and doings, until, from time to time, an advance is made in civilization or science, and we find that this or that doctrine and practice, with all its attendant sufferings and inhumanities, was in reality utterly unnecessary, and utterly uncalled for.‡ In gene-

* All writers on surgical history give more or less full details upon this opposition to the practice of Paré. Thus, for example, Professor Cooper observes, "By many surgeons, however, the tying of arteries continued to be deemed too troublesome, and hence they persisted in the barbarous use of the actual cautery; of this number were Pigrai, F. Plazzoni, and P. M. Rossi. Nay, so difficult was it to eradicate the blind attachment shown to the ancients, that Theodorus Baroni-
nius, a professor at Cremona, publicly declared, in 1609, that he would rather err with Galen than follow the advice of any other person. . . . I shall not here expatiate upon the ill-treatment which Paré experienced from the base and ignorant Gourmelin, president of the Parisian college of physicians; nor upon the slowness and reluctance with which the generality of surgeons renounced the cautery for the ligature. . . . Almost 100 years after Paré, a button of vitriol was ordinarily employed in the Hotel Dieu at Paris for the stoppage of hemorrhage after amputations; Dionis was the first French surgeon who taught and recommended Paré's method. This happened towards the close of the 17th century, while Paré lived towards the end of the 16th."—*Cooper's Dictionary of Practical Surgery*, 7th edit. pp. 46, 47. See also *Sprengel's Histoire de Médecine*, vol. iii. p. 315; *Bell's Surgery*, vol. i. p. 226, &c.

† *Principles of Surgery*, vol. i. p. 212.

‡ Witness, for example,—(as compared with the *past* opinions of those who practised them)—our *present* opinions regarding the burning, by our Druidical forefathers, of whole wickerfuls of human living beings, and in the name of religion; or, in times nearer to our own—in Christian times—the application of the

ral, however, long years elapse before this new aspect in matters is duly seen; or, at least, duly acknowledged. While the practices themselves are in full operation, the mind, enthralled by education and habit, cannot be easily made to view them in their true character; and when, in the progress of the march of knowledge and science, their propriety and perpetuation come at last to be challenged and contested, human passions and prejudices ever (as in the above instance of cauterization) rise up to argue for, and insist upon, the continuance and safety of the past, and the total impolicy and high peril of any attempted alteration. But time passes on, and brings with it, sometimes abruptly—generally almost imperceptibly—a perfect change of doctrine and practice. Any surgeon who, in the days of Paré, dared to arrest the hemorrhages from his amputation wounds, by applying ligatures *instead* of red-hot irons, would have been denounced by his compeers. Any surgeon, on the contrary, who now, at this present day, dared to arrest the hemorrhages from his amputation wounds, by applying to the bleeding vessels, not ligatures but red-hot irons, would as certainly be denounced by his compeers, and his talents, as well as his humanity, would be strongly challenged. We look back with sorrow upon the pitiless practices in that respect of the contemporaries and opponents of Paré. In the course of years our successors in the profession will, I most sincerely believe, look back with similar feelings upon the alleged “insignificance,” and “propriety,” and “desirability” of pain in surgical operations, as maintained by many members of the profession at the present day; and they will equally marvel at the idea of men—of humane men—complacently confessing and upholding, that they prefer operating upon their patients in a waking instead of an anæsthetic state; and that the fearful agonies which they thus inflict—the agonies of the surgeon’s knife—should be endured rather than avoided—quietly and decorously submitted to, and not attempted to be eschewed. I have elsewhere discussed,* at some length, the strange opinions and practices of some modern surgeons, upon this alleged propriety and neces-

fire and faggot by man to man, still under the plea of religion; or the use of the rack and torture; the incrimination in the sixteenth and seventeenth centuries of many poor wretches for the alleged crime of witchcraft; the altered existing ideas regarding the required frequency of capital punishments, and the whole question regarding their policy; the recent rapid and complete change of doctrine regarding the horrors and inhumanity of slavery; the changes in practice regarding insanity from what it was in the last century, when chains and a dungeon were the portion of every poor lunatic, &c., &c.

* See Monthly Journal of Medical Science for September, 1847, pp. 156-166, “On the Allegation of the Prevention of Pain in Surgical Cases being unnecessary and Improper.”

sity of pain in surgical practice and surgical operations. On the present occasion, my object is to offer some remarks regarding the pains attendant upon parturition, and the propriety of alleviating and annulling the sufferings of our patients in obstetrical practice and obstetrical operations. But let me first adduce some evidence of *their* intensity and amount.

"The distress and pain (observes Dr. Denman)* which women often endure while they are struggling through a difficult labour are *beyond* all description, and seem to be more than human nature would be able to bear under any other circumstances." But even the amount of agony endured in most cases of *natural* parturition, is abundantly severe.† Viewed apart, and in an isolated light, the total sum of actual pain attendant upon common labour is as great, if not greater, than that attendant upon most surgical operations. It is, I believe, education and custom, and perhaps the idea of its inevitable necessity, which have made the profession in general look upon the degree of maternal pain and physical suffering accompanying natural parturition, as less deserving of consideration than in reality it is. These circumstances have, in a great measure, blinded us as to its actual amount, and intensity, and importance. For it was, no doubt with perfect truth, remarked by an author‡ who wrote three hundred years ago, "Mulier, in partu, maximos et fere intolerabiles sustinet dolores."

"This (observes Dr. Rigby) is the moment of greatest pain, and the patient is frequently quite wild and frantic with suffering; it approaches to a species of insanity, and shows itself in the most quiet and gentle dispositions. The laws in Germany have made great allowances for any act of violence committed during these moments of frenzy, and wisely and mercifully consider that the patient at the time was labouring under a species of temporary insanity. Even the act of child murder, when satisfactorily proved to have taken place at this moment, is treated with considerable leniency. This state of mind is sometimes manifested in a slighter degree by actions and words so contrary to the general habit and nature of the patient, as to prove that she could not have been under the proper control of her reason at the moment. It is a question how far this state of mind may arise from intense suffering, or how far the circulation of the brain may be affected by the pressure which is exerted upon the abdominal viscera."§

* Introduction to Midwifery, 5th edition, p. 377.

† Cases undoubtedly ever and anon occur, in which the mother suffers comparatively little or no pain; but these are exceptions, rare exceptions, to a general rule.

‡ Hieronymus Mercurialis, in Spachius Gynæcia, p. 233.

§ System of Midwifery, p. 103.

Such is the description of the amount of pain and agony endured in natural parturition, given by some of our best and most esteemed authorities in obstetric literature.

Is it right for the physician to interfere with these fearful sufferings and agonies in order to save and shield his patients from the endurance of them? Is it proper for him to exercise the skill of his art so as to moderate and remove these "almost intolerable pains (*fere intolerabiles dolores?*") Would it be fit and meet in him to use human means to assuage the pangs and anguish attendant upon the process of parturition in the human mother?

These questions, and questions like these, I have often during the currency of the present year, heard complacently put by medical men,—men, too, whose opinions and actions in other matters, and in other respects, were fully and truly actuated by that great principle of emotion which both impels us to feel sympathy at the sight of suffering in any fellow-creature,* and at the same time imparts to us delight and gratification in the exercise of any power by which we can mitigate and alleviate their suffering. Such questions, I repeat, are seriously asked by physicians and surgeons, the professed object of whose whole science and art is the relief of human disease and human suffering. They are questions propounded with all imaginable gravity and seriousness by individuals who (in a mere abstract point of view) would, no doubt, strongly object to being considered as anxious to patronise and abet human misery, or traffic in the perpetuation of human pain. Nay, probably, at the date at which I write, there is not one in twenty—perhaps not one in a hundred—of the physicians and surgeons of Great Britain who have, as yet, thought seriously upon the propriety of alleviating and annulling the tortures attendant on human parturition; or who have acknowledged to their own minds the propriety of their bestirring themselves so as to be able, in the exercise of their profession, to secure for their patients an immunity from the throes and agonies of childbirth.

Perhaps, as an apology for their indolence and apathy, some may be ready to argue, that the pain and suffering attendant on parturition is not dangerous and destructive in its results, however agonizing and distressing it may be to the patient during its continuance. But the argument is fundamentally unsound. All pain is *per se*, and especially when in excess, destructive and even ultimately fatal in its action and effects. It "exhausts (says Mr. Travers) the principle of life."† "It

* "Inditus est, ab ipsa Natura, homini, *miseriæ affectus nobilis et excelsus.*" Bacon—"De Augmentis Scientiæ," Lib. viii. cap. ii.

† Inquiry concerning Constitutional Irritation, vol. i. p. 76.

exhausts (says Mr. Burns of Glasgow) both the system and the part."* "Merc pain (observed the late Dr. Gooch) *can* destroy life."† And the great pain accompanying human parturition is no exception to this general pathological law. For, in fact, the maternal mortality attendant upon parturition, regularly increases in a ratio progressive with the increased duration of the woman's sufferings. The statistical data published by Dr. Collins, in his Report of the Dublin Lying-in Hospital, affords ample proof of this general principle. According to calculations which I some time ago made from Dr. Collins's data, I found that while in the women delivered in the Dublin Hospital, and whose sufferings were terminated within 2 hours, only 1 in 320 of the mothers died; where the labour varied in duration from 2 to 6 hours, 1 in 145 of the mothers died; in those in whom it continued from 7 to 12 hours, 1 in 80 died; where it endured from 12 to 24 hours, 1 in 26 died; where it lasted from 24 to 36 hours, 1 in 17 died; and out of all those whose parturient sufferings were prolonged beyond 36 hours, 1 in every 6 perished.

Again, some may possibly be inclined to reason, that any means by which we could produce a state of anæsthesia or insensibility to the physical pains of labour, must, of necessity, be of such a character as to add to the perils and dangers of the patient. I believe this argument to be as futile and untenable as the one that I have just noticed. Indeed, judging from analogy, and from what is the fact in surgery, I believe that, as a counteraction to the morbid influence of pain, the state of artificial anæsthesia does not only imply a saving of human suffering, but a saving also of human life. Out of above 300 cases of the larger amputations performed during the current year, upon patients in an etherized or anæsthetic state, and which I have collated from different hospitals in Great Britain, Ireland, and France, a smaller proportion died than formerly used to perish in the same hospitals under the same operations without etherization. I shall take one of these amputations as an illustration of the whole—and that one the most severe of all—viz., amputation of the thigh. Malgaigne (1842) showed, that under amputations of the thigh, in the hospital of Paris, 62 in every 100 died; in Edinburgh, the mortality from this operation, in the only years during which the hospital reports were published (1839–42), was 50 in every 100; Mr. Phillips of London (1844), found the average mortality 40 in 100; Dr. Lawrie at Glasgow (1839), found it also in the hospitals of

* Principles of Surgery, vol. i. p. 502.

† Dr. Merriman's Synopsis of Parturition, p. 239.

that city to be 40 in 100.* I have notes of 135 cases in which this same operation has been performed in hospital practice upon patients in an etherized state. Out of these 135 cases 33 died, or only 24 in 100. Hence I repeat, that the condition of anæsthesia not only preserves the patient in surgical practice from agony and torture, but actually preserves him too from the chances of danger and death. And I firmly believe, that the superinduction of anæsthesia in obstetric practice will yet be found to diminish and remove also, in some degree, the perils as well as the pains of labour.

In an essay which I wrote in February last, "On the Employment of the Inhalation of Sulphuric Ether in the Practice of Midwifery," (*Monthly Journal of Medical Science* for March 1847, p. 728,) I offered some remarks on its application to cases of common as well as of morbid parturition, and took occasion to observe, "The question which I have been repeatedly asked is this—Will we ever be 'justified' in using the vapour of ether to assuage the pains of natural labour? Now, if experience betimes goes fully to prove to us the safety with which ether may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured, I believe that the question will require to be quite changed in its character. For, instead of determining in relation to it whether we shall be 'justified' in using this agent under the circumstances named, it will become, on the other hand, necessary to determine whether on any grounds, moral or medical, a professional man could deem himself 'justified' in withholding, and not using any such safe means (as we at present presuppose this to be), provided he had the power by it of

* The following table exhibits the actual number of the cases of amputation of the thigh referred to in the text, with their respective results:—

Mortality Accompanying Amputation of the Thigh.

Name of Reporter.	No. of Cases.	No. of Deaths.	Per Cent. of Deaths.
Malgaigne, Paris,	201	126	62 in 100
Peacock, Edinburgh,	43	21	50 in 100
Phillips's Collection of Cases,	660	263	40 in 100
Lawrie, Glasgow,	184	73	40 in 100
Total,	1088	483	44 in 100
Upon Patients in an Anæsthetic state, .	135	33	24 in 100

assuaging the pangs and anguish of the last stage of natural labour, and thus counteracting what Velpeau describes 'as those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,'* which accompany the termination of natural parturition in the human mother."

Since the latter part of January, I have employed etherization, with few and rare exceptions, in every case of labour which has been under my care. And the results, as I have already elsewhere stated, have been, indeed, most happy and gratifying. I never had the pleasure of watching over a series of more perfect or more rapid recoveries; nor have I once witnessed any disagreeable result to either mother or child. I have kept up the anæsthetic state during periods varying from a few minutes to three, four, five, and six hours. I do not remember a single patient to have taken it who has not afterwards declared her sincere gratitude for its employment, and her indubitable determination to have recourse again to similar means under similar circumstances. All who happened to have formerly entertained any dread respecting the inhalation, or its effects, have afterwards looked back, both amazed at, and amused with, their previous absurd fears and groundless terrors. Most, indeed, have subsequently set out, like zealous missionaries, to persuade other friends to avail themselves of the same measure of relief in their hour of trial and travail; and a number of my most esteemed professional brethren in Edinburgh have adopted it with success, and results equal to my own. All of us, I most sincerely believe, are called upon to employ it by every principle of true humanity, as well as by every principle of true religion.† Medical men may oppose for a time the superinduction of anæsthesia in parturition, but they will oppose it in vain; for certainly our patients themselves will force the use of it upon the profession. The whole question is, even now, one merely of time. It is not—Shall the practice come to be generally adopted? but, When shall it come to be generally adopted? Of course, it will meet from various quarters with all due and determinate opposition. Medical men will, no doubt, earnestly argue that their established medical opinions and medical practices should not be harshly interfered with by any violent innovations of doctrine regarding the non-necessity and non-propriety of maternal suf-

* *Traité des Accouchemens*, vol. i. p. 449. "Ces cris perçans, cette agitation si vive, ces efforts excessifs, ces angoisses inexprimables, ces douleurs qui paraissent intolérables," &c.

† See "Answer to the Religious Objections urged against the employment of Anæsthetic Agents in Midwifery and Surgery."

fering. They will insist on mothers continuing to endure, in all their primitive intensity, all the agonies of childbirth, as a proper sacrifice to the conservatism of the doctrine of the desirability of pain. They will perhaps attempt to frighten their patients into the medical propriety of this sacrifice of their feelings;* and some may be found who will unscrupulously ascribe to the new agency any misadventures, from any causes whatever, that may happen to occur in practice. But husbands will scarcely permit the sufferings of their wives to be perpetuated merely in order that the tranquillity of this or that medical dogma be not rudely disturbed. Women themselves will sometimes rebel against enduring the usual tortures and miseries of childbirth, merely to subserve the caprice of their medical

* We can all recollect the many absurd stories of apocryphal disasters and deaths that the opponents of etherization busily and anxiously reported towards the commencement of the present year, as having occurred from the employment of ether-inhalation in surgery. Dr. Forbes, in his excellent article on etherization, in treating of these unscrupulous and disreputable pieces of professional gossip, observes—"One day we had death from asphyxia; another from coma; another from hæmoptysis; some from convulsions; a few from pneumonia; and one or two from actual incineration, or explosion, through the accidental firing of the ethereal vapour within the air-passages. We have not had time to investigate all these terrible cases, but we may state that we traced *the one* which seemed the *best* authenticated—that from hæmoptysis—from its full-blown majesty in after-dinner gossip, to its humble source in the hospital. And this was the case, as the man himself detailed it to us:—A day or two after a successful operation for hernia, under etherization, the man pricked his gums while picking his teeth with a pin; and it was the product of *this* operation, not of the ether, seen in the spitting-pot by the patient's bedside, that was bruited about town, as of itself sufficient to settle the question in all future time!"—(*British and Foreign Medical Review*, No. xlvii., April, 1847, p. 564).—When first employing etherization in midwifery, I met with no small number of similar strange tales and accusations. For example, in February last, a patient who happened to be severely frightened, had, in consequence, a premature labour. The child presented preternaturally; and died a day or two after birth. The mother was attacked with phlegmasia dolens, and made a very long and protracted recovery. Various kind friends, anxious about the results of etherization in midwifery, warned me of the professional odium which this case was bringing upon the new practice, and of the strong argument which it was affording to others against the safety of ether-inhalation in obstetrics. I was repeatedly and credibly told that ladies had informed their physicians, that the quantity used was so great that they had felt the odour of it perfectly oppressive when calling, even days afterwards, at the house of my patient. The answer to all this was sufficiently simple. The danger of death to the child from its prematurity and preternatural presentation appeared to be from the first so imminent, that I did not choose to peril the character of the new practice by following it in this case. The ether had not only not been used, but not a drop of it had ever been in the house. One of my patients was zealously attempted, some months ago, to be persuaded against the "horrors of ether," on the strong and round assertion, that some dozen ladies or more in Dublin, upon whom the practice had been tried, had indubitably perished from the effects of it. Unfortunately for the veracity of this statement, ether-inhalation had never once been used, or attempted to be used in obstetric practice in Dublin, up to that date, or for a long time afterwards. Indeed, the first case in which ether was employed in midwifery in Dublin only occurred this week (28th Nov.) as I am informed in a letter of that date, which I have just received regarding it, from Dr. Tyler.

attendants. And I more than doubt if any physician is really justified, on any grounds, medical or moral, in deliberately desiring and asking his patients to shriek and writhe on in their agonies for a few months—or a few years longer—in order that, by doing so, they may defer to his professional apathy, or pander to his professional prejudices.

Two agents have the power of producing anæsthesia during labour, viz., the inhalation of sulphuric ether, and the inhalation of chloroform. With most, if not all, of my professional brethren, I believe that the latter agent possesses various important advantages over the former, particularly in obstetric practice; and that, in particular, it is far more portable; more manageable and powerful; more agreeable to inhale; is less exciting than ether; and gives us far greater control and command over the superinduction of the anæsthetic state. In the remaining part of these observations I shall detail briefly some instances illustrative of its effects and utility in the production of anæsthesia in cases of natural and morbid parturition.

CASE I.—The patient to whom it was first exhibited had been previously delivered in the country by craniotomy after a very long labour. Her second confinement took place a fortnight before the full time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded, and the pains very severe. In twenty-five minutes the child was born. The crying of the infant did not rouse the mother, nor did she awake till after the placenta was removed. She was then perfectly unaware that her child was born. She stated her sensations to be those of awaking from “a very comfortable sleep.” It was, for a time, a matter of no small difficulty to persuade her that the labour was over, and that the living child presented to her was her own.

CASE II.—I exhibited it, with Mr. Carmichael, to a patient who had, at her preceding confinement, been in severe labour for twenty hours—followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body, and bed-clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born, till she suddenly heard it squalling at its first toilet in the next room. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.

CASE III.—Patient unmarried. A first labour. Twins. The first child presented by the pelvis, the second with the hand and

head. The chloroform was exhibited when the os uteri was nearly fully dilated. The passages speedily became greatly relaxed (as has happened in other cases placed under its full influence); and in a few pains the first child was born, assisted by some traction. I broke the membranes of the second, pushed up the hand, and secured the more complete presentation of the head. Three pains expelled the child. The mother was then bound up; her clothes were changed; and she was lifted into another bed. During all this time she slept on soundly, and for a full hour afterwards; the chloroform acting in this, as in other cases of its prolonged employment, as a soporific. The patient recollected nothing from the time of the first inhalations; and was in no small degree distressed when not one—but two—living children were brought by the nurse to her. Dr. Christison accompanied me to this case.

CASE. IV.—Primipara of full habit. When the first examination was made, the passages were rigid, and the os uteri difficult to reach. Between six and seven hours after labour began, the patient, who was complaining much, was apathized with chloroform. In about two hours afterwards, the os uteri was fully dilated, and in four hours and a half after the inhalation was begun, a large child was expelled. The placenta was removed, and the patient bound up and dressed before she was allowed to awake. This patient required an unusual quantity of chloroform; and Dr. Williamson, who remained beside her, states to me in his notes of the case, “the handkerchief was moistened often in order to keep up the soporific effect. On one occasion, I allowed her to emerge from this state for a short time; but on the accession of the first pain she called out so for the chloroform, that it was necessary to pacify her by giving her some immediately. In all, four ounces of chloroform were used.” Like the others, she was quite unconscious of what had gone on during her anæsthetic state; and awoke altogether unaware that her child was born.

CASE V.—Second labour. This patient, after being several hours in labour, was brought to the Maternity Hospital. I saw her some time afterwards, and found the first stage protracted by the right side of the cervix uteri being thick, œdematous, and undilatable. The inhalation of chloroform was begun, and the first stage was terminated in about a couple of hours. Two or three pains drove the child through the pelvic canal, and completed the second stage. Fifteen minutes in all elapsed from the termination of the first to the termination of the third stage, or the expulsion of the placenta. The patient was dressed and removed into a dry bed, where she slept on for a short time before awaking, and being conscious of her delivery.

CASE VI.—Second labour. The patient, a person of small form and delicate constitution; bore her first child prematurely at the seventh month. After being six hours in labour, the os uteri was fully expanded and the head well down in the pelvic cavity. For two hours subsequently it remained fixed in nearly the same position, and scarcely if at all advanced, although the pains were very distressing, and the patient becoming faint and exhausted. She entertained some mistaken religious feelings against ether or chloroform, which had made her object to the earlier use of the latter; but I now placed her under its influence. She lay as usual like a person soundly asleep under it, and I was now able, without any suffering on her part, to increase the intensity and force of each recurring pain, by exciting the uterus and abdominal muscles through pressure on the lower part of the vagina and perineum. The child was expelled in about fifteen minutes after the inhalation was commenced. In a few minutes she awoke to ask if it was really possible that her child had been born; and was overjoyed to be told that it was so. I had the conviction that in this case the forceps would in all probability have been ultimately required, perhaps hours subsequently, provided I had not been able to have interfered in the way mentioned. I might, it is true, have followed the same proceeding though the patient was not in an anæsthetic state, but I could not have done so without inflicting great misery and agony upon her, and meeting with great resistance.

CASE VII.—A third labour. The patient had been twice before confined of dead premature children; once of twins, under the care of Mr. Stone of London; the second time of a single child, under my own charge. The liquor amnii began to escape about one o'clock, A.M., but no pains followed for some time. I saw her between three and four, with the pains commencing, and the os uteri beginning to dilate. In two hours afterwards the first stage was well advanced, and, the pains becoming severe, she had the chloroform exhibited to her, and slept soundly under its influence. In twenty minutes the child was born, and cried very loudly without rousing the mother. In about twelve or fifteen minutes more she awoke, as the application of the binder was going on, and immediately demanded if her child was really born and alive, as she thought she had some recollection of hearing the nurse say so. She was rejoiced beyond measure on her son being brought in and presented to her.

CASE VIII.—Fourth labour. The patient had borne three dead children prematurely, about the sixth and seventh months of utero-gestation. During her present pregnancy I placed her

under strict rules and discipline ; and she used, from an early period, small doses of chlorate of potass several times a-day. She carried her children to the full time. Labour came on about one o'clock, A.M. The membranes broke at eight A.M., when the os uteri was still very slightly open. It had made very little progress to ten o'clock, when Dr. Keith exhibited the chloroform to her. The pains continued very strong and regular, the passages relaxed, and at half-past eleven she was delivered of a large living child. The placenta came away immediately ; and she was bound up, and her soiled clothes removed, before she awoke. She remembered nothing whatever that had occurred after she began to inhale the chloroform till the period of her awaking.

The preceding instances afford, perhaps, a sufficient number of examples of the use of chloroform in natural labour. In these and in all others which I have seen, or that have been reported to me, the immediate effects of the chloroform have been delightful. The mothers, instead of crying and suffering under the strong agonies and throes of labour, have lain in a state of quiet, placid slumber, made more or less deep at the will of the medical attendant, and, if disturbed at all, disturbed only unconsciously from time to time by the recurring uterine contractions producing some reflex or automatic movements on the part of the patient—like those of a person moving under any irritation of the surface, or from the touch of another, though still in a state of sleep. Nor have the ultimate consequences and results been less happy. No difficulties have been met with in the third stage ; and the uterus has contracted perfectly after delivery. I never saw mothers recover more satisfactorily or rapidly,—or children that looked more viable. And the practice is not a great blessing to the patient merely ; it is a great boon also to the practitioner. For whilst it relieves the former from the dread and endurance of agony and pain, it both relieves the latter from the disagreeable necessity of witnessing such agony and pain in a fellow-creature, and imparts to him the proud power of being able to cancel and remove pangs and torture that would otherwise be inevitable. It transforms a work of physical anguish into one of painless muscular effort ; and changes into a scene of sleep and comparative repose, that anxious hour of female existence, which has ever been proverbially cited as the hour of the greatest of mortal suffering.

The effects of the superinduction of anæsthesia in parturition are, if possible, still more marked and beneficial in cases of morbid labour and operative delivery. In proof of its influence in this respect, I shall cite some examples of its employment

in cases of turning, of the application of the forceps, and of embryulsio.

CASE IX.—Fourth labour. The mother deformed, and the conjugate diameter of the brim of the pelvis contracted from the projection inwards and forwards of the promontory of the sacrum. Her first child was delivered by embryulsio; the second by the long forceps; the third was small, and passed without artificial assistance. On the present occasion, after suffering slight pains during the whole night, labour set in with greater severity towards morning. After being in strong labour for some hours, she was seen first by Mr. Figg, and afterwards by Dr. Peddie, her ordinary medical attendant. I was called to her about four o'clock P.M. The pains were then enormously powerful and straining, imparting to the mind the dread of the uterus rupturing under their influence; but the head of the child was still altogether above the brim, and only an œdematous ridge of the scalp pressed through the superior and contracted pelvic opening. The passages had become heated, the mother's pulse raised, &c., and Dr. Peddie had tried two different pairs of long forceps. After I arrived he applied, with great skill, another pair of long forceps which I had with me; but it was found impossible to move the head in the least degree forwards. The urgency and power of the uterine contractions, the immobility of the head upon the brim of a deformed pelvis, and the state of the patient and of the parts, all showed the necessity of relief being obtained by artificial delivery. In her first labour I had assisted Dr. Peddie in delivering her under similar circumstances by perforation of the head. But here the child's heart was heard distinctly with the stethoscope, and he at once agreed to my proposition, that I should try to deliver her by turning the infant,—compressing and indenting the flexible skull of the fœtus, instead of perforating it, and thus affording (as I have for some time past taught and believed) some chance of life to the child, and more chance of safety to the mother. The patient was placed under the influence of chloroform still more deeply than when the forceps were used, in order, if possible, entirely to arrest the uterine contractions. I passed up my hand into the uterus, seized a knee, and easily turned the infant; but very great exertion and pulling was required to extract the child's head through the distorted brim. At last it passed, much compressed and elongated. The child was still-born, but, by applying the usual restorative means, it speedily began to breathe and cry. The child continues well, and the mother has made a rapid recovery.

CASE X.—In the Maternity Hospital; first child. Labour

began at ten P. M. (21st Nov.) I was desired to see her at six A. M. (22d). The os uteri was well dilated, but it was evident that the pelvic canal was contracted throughout, and the head was passing with unusual difficulty through the brim. The patient was complaining much of her sufferings. It was clear that it would be a very tedious and probably at last an instrumental case, and one therefore calculated to test the length of time during which chloroform might be used. She began to inhale it at a quarter past six A. M., and was kept under its influence till a quarter past seven P. M., the date of her delivery; thirteen hours in all. From the time it was begun to the time delivery was completed, her cries and complaints ceased, and she slept soundly on throughout the day. The bladder required to be emptied several times with the catheter. The head passed the os uteri at ten A. M.; and, during the day, gradually descended through the pelvis. At seven A. M. I at last deemed it proper to deliver her by the forceps; the head, which was now elongated and œdematous, having by that time rested for some hours against the contracted pelvic outlet with little or no evidence of advancement, the bones of the foetal cranium overlapping each other, and the foetal heart becoming less strong and distinct in its pulsations. A warm bath, irritation of the chest, &c., were necessary to excite full and perfect respiration in the infant. Whilst we were all busied with the infant the mother lost some blood; but the placenta was immediately removed, and the uterus contracted perfectly. On afterwards measuring the quantity of blood lost, it was calculated to amount to 15 or 18 ounces. The mother's clothes were changed; she was bound up and removed to a dry bed before she awoke. She had at first no idea that the child was born, and was in no respect conscious of being delivered. In fact she had been "sleeping," according to her own account, from the time she had begun the inhalation, and only thought she once or twice remembered or dreamed that she heard Dr. Williamson, the house surgeon, speak near her. Dr. Beilby, Dr. Zeigler, &c., saw the case with me. The mother and child have continued perfectly well.

In this, as in other cases, I have watched and noted the effects of the chloroform upon the duration of the pains and of the intervals, the rate of the foetal and maternal pulse, &c.

CASE XI.—Patient with a deformed spine and contracted pelvic outlet. At her first confinement two different medical gentlemen had failed in effecting delivery by the forceps. At this her second confinement, she placed herself under the care of Dr. Paterson of Leith. After being very long in labour, and the symptoms of the case becoming urgent, I saw her with Dr.

Paterson. The head was low down in the pelvis; but it was placed in the right-occipito posterior position (the third of Nægele), and the forehead instead of the vertex was presenting, one orbit being easily felt behind the symphysis pubis. It had been lodged in nearly the same position for many hours. The foetal heart was still distinct, but weak. I applied the forceps—turned the head round with them a quarter of a circle, into an occipito-anterior position (the second of Nægele); and, after being so adjusted, it still required considerable force to extract it. Before applying the forceps the patient was sent into a state of deep anæsthesia by the inhalation of chloroform; and subsequently, when she wakened out of it, she was in no small degree surprised to find that she had really been delivered while she was sleeping and resting so soundly. The placenta separated, and the uterus contracted firmly. The child, which was large, lived for eight hours after delivery; but, despite of all the measures tried, full and perfect respiration was never established in it, apparently in consequence of some effusion or injury about the base of the brain. Unfortunately a post-mortem examination was not obtained. The mother has made an excellent recovery.

I quote the following instance of craniotomy under chloroform from a letter (dated 29th November), which I have received from my friend, Professor Murphy of London. I give the case in Dr. Murphy's own words:—

CASE. XII.—“I have tried the chloroform with great success in a case of distorted pelvis. It was the ovate deformity, the conjugate measurement being only $2\frac{1}{2}$ inches; the head of the child could not enter the brim: and I was obliged to perforate. I got Dr. Snow to assist me in bringing her under the influence of chloroform. She made some resistance, and struggled a good deal at first, chiefly I think from apprehension that we were going to do something very dreadful; however she soon began to inhale quietly, and gradually fell into a kind of dreamy sleep. I perforated the head, and laboured with the crotchet (sometimes with the craniotomy forceps) for three quarters of an hour before I could get the head through the brim. She was at length delivered; the placenta was separated in about ten minutes; the bandage applied, soiled clothes removed; and she was made ‘clean and comfortable,’ as the midwives say. My patient was perfectly unconscious all this time, and did not awake for about a quarter of an hour after the operation: she did so then quite quietly, and was greatly surprised to find that all her miseries were over. There was no hemorrhage, but the uterus felt rather spongy and large. She is now recovering most favourably. I never had a case recover so far, so well.”

Other cases, both of natural and morbid labour, in which the patients were delivered in an anæsthetic state from the inhalation of chloroform, have been reported to me by Dr. Protheroe Smith, Dr. Imlach, Dr. Robertson of Birkenhead, Dr. Malcolm, Dr. Buchanan, &c.; but as these, and some other instances which I have myself seen, presented nothing new or different in their phenomena from the cases which I have already detailed, I have thought it unnecessary to give at present the details of them.

CHAPTER III.

ANSWER TO THE RELIGIOUS OBJECTIONS ADVANCED AGAINST THE EMPLOYMENT OF ANÆSTHETIC AGENTS IN MIDWIFERY AND SURGERY.

"For every creature of God is good, and nothing to be refused, if it be received with thanksgiving."—1st Timothy iv. 4.

"Therefore to him that knoweth to do good and doeth it not, to him it is sin."—James iv. 17.

ALONG with many of my professional brethren in Scotland, and perhaps elsewhere, I have, during the last few months, often heard patients and others strongly object to the superinduction of anæsthesia in labour, by the inhalation of Ether or Chloroform, on the assumed ground, that an immunity from pain during parturition was contrary to religion and the express commands of Scripture. Not a few medical men have, I know, joined in this same objection;* and have refused to relieve their patients from the agonies of childbirth, on the allegation that they believed that their employment of suitable anæsthetic means for such a purpose would be unscriptural and irreligious. And I am informed, that in another medical school, my conduct in introducing and advocating the superinduction of anæsthesia in labour has been publicly denounced *ex cathedra* as an attempt to contravene the arrangements and decrees of Providence, hence reprehensible and heretical in its character, and anxiously to be avoided and eschewed by all properly principled students and practitioners. I have been favoured with various earnest private communications to the same effect. Probably, therefore, I may be excused if I attempt, however imperfectly, to point out what I conscientiously conceive to be the errors and fallacies of those who thus believe that the practice in question ought in any degree to be opposed and rejected on religious grounds.

It is almost unnecessary to begin with premising, that those

* "Pain during operations is, in the majority of cases, even desirable; its prevention or annihilation is, for the most part, hazardous to the patient. In the lying-in chamber, nothing is more true than this; pain is the mother's safety, its absence her destruction. Yet, there are those bold enough to administer the vapour of ether, even at this critical juncture, forgetting it has been ordered, that 'in sorrow shall she bring forth.'"—(On the "*Injurious (?) Effects of the Inhalation of Ether*;" in *Edinburgh Medical and Surgical Journal* for July, 1847, p. 258.)

who object to the superinduction of anæsthesia in parturition upon religious grounds, found their objections principally on the words of the primeval curse which God pronounced after the temptation and fall of our first parents. Few or none, however, of those who have most zealously urged the existence of this curse as a reason against the employment of anæsthetic means in obstetric practice, have, I believe, made themselves at all intimate with the words and tenor of the curse itself. I shall, therefore, in the first place, quote the words of it in full from the third chapter of Genesis, interpolating in Roman letters the Hebrew originals of those two nouns which are the more immediate subjects of doubt and difference of opinion.

GENESIS, chap. iii. v. 14.—“And the Lord God said unto the serpent,—Because thou hast done this, thou art cursed above all cattle, and above every beast of the field; upon thy belly shalt thou go, and dust shalt thou eat all the days of thy life.

15. “And I will put enmity between thee and the woman, and between thy seed and her seed; it shall bruise thy head and thou shalt bruise his heel.

16. “Unto the woman he said, I will greatly multiply thy sorrow (*'itztzabhon*) and thy conception; in sorrow (*'etzebbk*) thou shalt bring forth children; and thy desire shall be to thy husband, and he shall rule over thee.

17. “And unto Adam he said,—Because thou hast hearkened unto the voice of thy wife, and hast eaten of the tree, of which I commanded thee, saying, Thou shalt not eat of it; cursed is the ground for thy sake: in sorrow (*'itztzabhon*) shalt thou eat of it all the days of thy life.

18. “Thorns also and thistles shall it bring forth to thee; and thou shalt eat the herb of the field.

19. “In the sweat of thy face shalt thou eat bread, till thou return unto the ground; for out of it wast thou taken: for dust thou art, and unto dust shalt thou return.”

In the form of a few separate observations, I will now add the remarks and answers which I wish to make. And I would begin by observing, that,—

1. The primeval curse is triple. It contains a judgment, First, upon the serpent (verses 14, 15); Secondly, upon the woman (v. 16); and, Thirdly, upon the ground for the sake of the man (v. 17–19).—With the first of these three curses—that on the serpent—and its apparent permanence (Isaiah lxxv. 25), our present inquiry has nothing to do. It is enough for me to remark, that the second and third curses—on the woman and on the ground—are evidently, from different parts of the Holy Word, not immutable. God himself, on more than one occasion, promises the removal of them, and in general conjunctly, to the Israelites, provided they would keep their covenants and obey his laws. See, for example, Deuteronomy vii. 13, “I will bless the fruit of thy womb, and the fruit of thy land,” &c.; xxviii. 4, “Blessed shall be the fruit of thy body, and the fruit of thy ground,” &c. See also chap. xxviii. 11, &c. In Isaiah (xxviii. 23–29), man’s culture by the plough, &c., of the

ground cursed by God, is said to come from the providence of God himself. "For his God doth instruct him to discretion, and doth teach him," (v. 26); and, "This also cometh forth from the Lord of hosts, which is wonderful in counsel and excellent in working" (v. 29).

2. Those who, from the terms of the first curse, argue against the superinduction of anæsthesia in labour, aver that we are bound to take and act upon the words of the curse *literally*, "I will greatly multiply thy sorrow and thy conception;" or as Gesenius and other Hebrew authorities state, that, being a case of Hendiadys, it may be *more* correctly rendered, "I will greatly multiply the sorrow *of* thy conception;* in sorrow thou shalt bring forth children." If, however, we are bound to take *this* part of the curse literally, and act accordingly, then we are bound to take and act also upon *all* other parts of the curse literally. If it is sinful to try to counteract the effects of this part of it, referring to child-bearing women, it is sinful to try to counteract the other parts of it, regarding the state of the ground, and the judgment upon man. The agriculturist, in pulling up "the thorns and thistles" which the earth was doomed to bear, so far tries to counteract that part of the primary doom; and yet is never looked upon as erring and sinning in doing so. Or grant, as I have heard argued, that he may be entitled to pull up "the thorns and thistles," because the curse further implies that he was doomed to till the ground,—still he was doomed to till it by "the sweat of his face." Now if, I repeat, the whole curse is, as is averred, to be understood and acted on literally, then man must be equally erring and sinning, when, as now, instead of his own sweat and personal exertions, he employs the horse and the ox—water and steam power—sowing, reaping, thrashing, and grinding machines, &c., to do this work for him, and elaborate the "bread" which he eats. The ever active intellect which God has bestowed upon man, has urged him on to the discovery of these and similar inventions. But if the first curse must be read and acted on literally, it has so far urged him on to these improper acts by which he thus saves himself from the effects of that curse. Nay, more; if some physicians hold that they feel conscientiously constrained not to relieve the agonies of a woman in childbirth, because it was ordained that she should bring forth in sorrow, then they ought to feel conscientiously constrained on the very same grounds not to use their professional skill and art to prevent man from dying; for at the same time it was decreed, by the same authority with the same force, that man should be subject to death,—"*dust thou art, and unto*

* "*Augebo tibi Graviditatis molestias.*"—Dathe's *Pentateuchus*, p. 38.

dust shalt thou return." If, on the other hand, it be allowed that it is justifiable in the physician to try to counteract the effects of one part of the curse, and justifiable in the agriculturist to try to counteract the effects of another part, it is surely equally justifiable in the accoucheur to try to counteract the effects of a third part of it. But if, on the contrary, it is unjustifiable for him to follow out this object of his profession, it is equally unjustifiable for the physician and agriculturist to follow out the corresponding objects of their professions. Are those who maintain the uncanonical character of using human means to contravene the pains of childbirth ready, then, to maintain that we should not use human means to contravene the tendency to death, or to increase the fertility and produce of the ground except by personal labour, and the actual "sweat" of the brow? To be consistent, they must of necessity maintain this strange and irrational view of man, and of the duties and destinies which God has appointed for man. Or, otherwise, they must own that if it is right and meet in us to exert the human intellect so as to ameliorate the condition of man from the results of the fall, it is equally right and meet in us to employ the same means to ameliorate the condition of woman from the results of the same cause.

3. But does the word sorrow ("in sorrow thou shalt bring forth children") really mean physical and bodily *pain*, as is taken for granted by those who maintain the improper and irreligious character of any means used to assuage and annul the sufferings of childbirth? Now, the word "sorrow" occurs three several times in two consecutive verses of the curse; (verses 16 and 17). The corresponding word, or rather words, in the original Hebrew, as I have already shown when citing the terms of the curse, are *'etzeb*, and *'itztzbhon*. These nouns are both synonymous in meaning and origin, although longer and shorter in form (like labour, laboriousness—pain, painfulness—in our own language). All philologists agree that they are derived from the same root, viz., the verb *'atzabh*. The true and primitive meaning of a derivative word in the Hebrew, as in other languages, is generally the best attained by considering the signification of the root from which it is derived. The meaning of the verb *'atzabh* (the root of these nouns) is given as follows, by Professor Gesenius, the highest authority, I believe, I could quote on such a point. In his *Lexicon* he enters "*'atzabh*, 1. To labour, to form, to fashion. The original idea (says he) is perhaps that of cutting, whether wood or stones. 2. To toil with pain, to suffer, to be grieved; used also of the mind" (Tregelles' Translation of Gesenius'

Hebrew and Chaldee Lexicon, p. DCXLVI). Of the disputed nouns, the noun *'etzeb* ("in sorrow—*'etzeb*—thou shalt bring forth children") is nearest in form, and hence in meaning, to the original verb-root *'atzab*—and, I believe, no scholar would deem it erroneous to affix to it the same simple original signification "*labour*," "*toil*," without deeming it requisite to believe, that it at all farther necessarily imports that the implied labour and effort must essentially be to such an excess as actually to amount to the supervention of pain and agony. In fact, the Hebrew word for *labour* (in the sense of work or toil) is exactly like the English word *labour*, used also to import the act of parturition. Certainly, the greatest characteristic of human parturition as compared with parturition in the lower animals, is the enormous amount of muscular action and effort (labour) provided for, and usually required for its consummation. The erect position (*vultus ad sidera erectus*) of the human body, renders a series of peculiar mechanical arrangements and obstructions necessary in the human pelvis, &c., for the prevention of abortion and premature labour, and for the well-being of the mother during pregnancy. But these same mechanical adaptations and arrangements (such as the angle at which the pelvis is set to the spine,—the great difference in the axis of the pelvic brim, cavity, and outlet—the rigidity of the soft structures, &c.) all render also, at last, the ultimate expulsion of the infant in labour, a far more difficult, and more prolonged process than in the quadruped, for instance, with its horizontal body. To overcome these greater mechanical obstacles, the human mother is provided with a uterus immensely more muscular and energetic than that of any of the lower animals. The uterus of woman is many times stronger and more powerful than the uterus, for example, of the cow. In other words, I repeat, the great characteristic of human parturition is the vastly greater amount of muscular effort, toil, or labour required for its accomplishment.* The state of anæsthesia does not withdraw or abolish that muscular effort, toil, or labour; for if so, it would then stop, and arrest entirely the

* In some of the black tribes of the human race, the muscular efforts and exertions of the uterus seem to be accompanied with comparatively little or no physical pain—there is labour *without* suffering. But the black woman was cursed as well as the white; and surely it cannot be irreligious to *reduce* the sufferings of the civilized female to the degree and amount which nature has left them existing in the uncivilized female of our race. There are abundance of "maternal sorrows" connected with children and child-bearing in the civilized woman, quite independently of the actual agonies of parturition. My friend Dr. Churchill of Dublin, some years ago, published a large octavo volume on the affections *peculiar* to the pregnant and puerperal states, without at all including those observable *during* labour.

act of parturition itself. But it removes the physical pain and agony otherwise attendant on these muscular contractions and efforts. It leaves the labour itself (*'atzebh*) entire. And in relation to the idea, that the Hebrew noun in the text truly signifies muscular *toil* and effort, and not physical *pain* and maternal agony, it is further highly important to remark, that in the very next verse (verse 17), viz., in the first part of the curse on man, the analogous Hebrew noun (*'itztabhon*), which we translate by "sorrow," assuredly does *not* in any degree mean or imply mortal suffering or pain, but toil and labour. "In sorrow thou shalt eat of it (the ground) all the days of thy life." Indeed, the very same noun (*'itztabhon*), when it occurs with the same meaning, and in relation to the same curse two chapters onwards—Genesis v. 29—is, in our own version, rendered by the word "toil," and not "sorrow." "And he called his name Noah (rest or comfort), saying, This same shall comfort us concerning our work or toil (*'itztabhon*) of our hands, because of the ground which our Lord hath cursed."

The word "sorrow" is a term at once simple and striking, but, at the same time, very comprehensive in its signification; and used under various specific meanings in our authorized English version of the Bible. In the Old Testament above twenty different terms or nouns in the original Hebrew text, are translated by the single term or noun "sorrow" in the English text.* And perhaps it may not be considered irrelevant, if I remark, that the identical Hebrew noun *'etzebh*, translated "sorrows" in the 16th verse ("in sorrow—*'etzebh*—thou shalt bring forth children"), recurs in six, and I believe only in six, other passages in the Old Testament; and in not one of these does it certainly imply physical pain. In two of these six places it is rendered, in our English version, by the very word "labour," in the signification of toil or work,—viz., in Prov. xiv. 23, "In all labour (*'etzebh*) there is profit;" and Prov. v. 10, "Lest thy labours† (*'etzebh*) be in the house of a stranger." In one passage it is translated "anger,"‡ Prov. xv. 1, "Grievous words stir up anger (*'etzebh*)." In another passage in which it occurs in Prov. x. 22, it is rendered sorrow, but still in the sense of toil and work—"The blessing of the Lord, it maketh rich, and he addeth no sorrow (*'etzebh*)|| with it." In Psalms cxxvii. 2, it is also, in our English version, translated "sorrows"—"It is in vain for you to rise up early, and sit up late, to eat

* See a list of these various Hebrew words which the translators of the English Bible have rendered by the word "sorrow," in "The Englishman's Hebrew and Chaldee Concordance of the Old Testament," p. 1639.

† "Labours," i. e. "things done with toil."—*Gesenius*.

‡ "A word pronounced with anger—a bitter, sharp word."—*Gesenius*.

|| That is, no "heavy and toilsome labour."—*Gesenius*.

the bread of sorrow (*'atzabhim*, the plural of *'etzeb*).”* And, lastly, in Jeremiah xxii. 28, the same noun is translated “idol” (a thing made, worked, or fashioned), “Is this man Coriah a despised, broken idol (*'etzeb*)?”

The context, I repeat, in these six Biblical passages in which the noun *'etzeb* recurs, shows that in *them* the word is not, in any respect, employed to designate the *sensation* of pain which accompanies the act of parturition in the human female. And it is surely not an unfair, or illegitimate deduction, to infer that in the only *one* remaining, or seventh instance in which the word occurs in the Bible—viz., in Genesis iii. 16—it would be used in the sense in which it is generally elsewhere used—of effort, toil, or labour—and not in a new sense, in which it is nowhere else used—of the *feeling* or perception of excruciating suffering, or bodily anguish.

4. But that the preceding deduction is sound and just, admits of additional, and still stronger corroborative evidence. In various passages in the Bible, the proverbial agony and pain of a woman in travail is brought in—and particularly in the inspired language of the Prophets—as a striking and beautiful simile, to mark the greatest possible degree of anguish and suffering. In not one of these passages, in which the pure pain and super-sensitive suffering of the parturient mother are thus referred to, is the word in Genesis iii. 16, viz.,—the word *'etzeb*—employed to designate this feeling of pain and suffering. Two other and totally different Hebrew nouns are used for this purpose in the passages to which I allude. These two nouns are *hhil* and *hhebbel*. They mark and designate the sensations of agony accompanying parturition, as contradistinguished from the muscular efforts (or labour) (*'etzeb*) in which the physiological part of the process of the expulsion of the child essentially consists. To illustrate the particular signification thus attached to the words *hhil* and *hhebbel*, as contradistinguished from *'etzeb*, I will cite the passages in which the two former nouns are used. In the following instances, the noun *hhil* is translated “pain,” “pangs,” &c.:—Psalm xlviii. 6, “Fear took hold upon them there, and pain as of a woman in travail.” Jeremiah vi. 24, “Anguish hath taken hold of us, and pain as of a woman in travail.” Jeremiah xxii. 23, “When pangs come upon thee, the pain as of a woman in travail.” See, also, Jeremiah l. 43. Micah. iv. 9, “Now why dost thou cry out aloud? is there no king in thee? is thy counsellor perished? for pangs have taken thee as a woman in travail.” In the following instances, the noun *hhebbel* occurs in the original Hebrew with the same meaning attached to it:—Isaiah

* “Bread obtained by toilsome labours.”—*Gesenius*.

xiii. 8, "Pangs and sorrow shall take hold of them; they shall be in pain as a woman that travaileth." Isaiah xxvi. 17, "Like as a woman with child, that draweth near the time of her delivery, is in pain and crieth out in her pangs." See, also, Isaiah lxvi. 7; Jeremiah xiii. 21, and xlix. 23. Hosea xiii. 13, "The sorrows of a travailing woman shall come upon thee."

From what I have stated under the two preceding heads, we are then, I believe, justly entitled to infer that the Hebrew term which, in our English translation of the primeval curse, is rendered "sorrow" (Genesis iii. 16), principally signifies the severe muscular *efforts* and *struggles* of which parturition—and more particularly human parturition—essentially consists; and does not specially signify the *feelings* or *sensations* of pain to which these muscular efforts or contractions give rise.—And, 2. On the other hand, the *feelings* or *sensations* of excruciating pain accompanying the process of parturition, are designated throughout the Bible by two Hebrew words which are entirely and essentially different from that term which is translated "sorrow," in the oft-repeated expression—"in sorrow thou shalt bring forth children."

5. But even if—contrary to what, I think, the whole philological consideration of the very terms and words of the Bible shows to be the case—we were to admit that woman was, as the results of the primal curse, adjudged to the miseries of pure physical pain and agony in parturition, still, certainly under the Christian dispensation, the moral *necessity* of undergoing such anguish has ceased and terminated. Those who believe otherwise, must believe, in contradiction to the whole spirit and whole testimony of revealed truth, that the death and sacrifice of Christ was not, as it is everywhere declared to be, an all-sufficient sacrifice for all the sins and crimes of man. Christ, the "man of sorrows," who "hath given himself up for us an offering and a sacrifice to God," "surely hath borne our griefs and carried our sorrows;" for God "saw the travail of his soul, and was satisfied." And He himself told and impressed on his disciples, that His mission was to introduce "mercy, and not sacrifice."—(See Matthew ix. 13; xii. 7; also Hos. vi. 6.) At the end of his commentary upon the curse in the third chapter of Genesis, the sound and excellent Matthew Henry, in his own quaint, pithy, and zealous style, justly observes, "How admirably the satisfaction our Lord Jesus Christ made by His death and sufferings, answered the sentence here passed upon our first parents. 1. Did *travailing pains* come in with sin? We read of the 'travail of Christ's soul;' Isa. liii. 11; and the pains of death he was held by, are called *adivai*, Acts ii. 24,—the 'pains of a woman in travail.' 2. Did *subjection* come in

with sin? Christ was 'made under the law;' Gal. iv. 4. 3. Did the *curse* come in with sin? Christ was made 'a curse for us;' died a 'cursed death;' Gal. iii. 13. 4. Did *thorns* come in with sin? He was crowned with 'thorns' for us. 5. Did *sweat* come in with sin? He sweat for us, 'as it had been great drops of blood.' 6. Did *sorrow* come in with sin? He was 'a man of sorrows;' his soul was in agony 'exceeding sorrowful.' 7. Did *death* come in with sin? He became 'obedient unto death.' Thus is the plaister as wide as the wound. Blessed be God for Jesus Christ."—(*Exposition of the Books of Moses*, p. 19.)

6. It may not be out of place to remind those who oppose the employment of anæsthetic means in labour on supposed religious grounds, that on the very same grounds many discoveries in science and art,—even in the medical art—have been opposed upon their first proposition; and yet, *now* that their first introduction is over, and the opinions and practices they inculcate are established, no one would be deemed exactly rational who would turn against the present or future *continuance* of their employment any such improper weapon. I might adduce many instances, but one may suffice for all. When small-pox inoculation was introduced towards the commencement of the last century, Rev. Mr. Delafaye and Mr. Massey published sermons against the practice as indefensible, on religious as well as medical grounds.* Inoculation was declared a "diabolical operation," and a discovery sent into the world by the Powers of Evil. And, again, when Dr. Jenner introduced vaccination instead of small-pox inoculation, towards the commencement of the present century, theological reasons again were not wanting for calling in question the orthodoxy of this other new practice. "Small-pox (argued Dr. Rowley) is a visitation from God, and originates in man, but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the con-

* See Delafaye's sermon on "Inoculation; an Indefensible Practice."—Massey's "Sermon against the Dangerous and Sinful Practice of Inoculation." In his admirable "Account of the Inoculation of Small-pox in Scotland (1765)," Dr. Mouro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a heinous crime." P. 5. "Clergymen (observes Dr. Baron, in his *Life of Jenner*, vol. i. p. 231), preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things (he adds) would scarcely obtain credence were it not that similar arguments and assertions have been employed against vaccination itself."

templative and learned ministers of the gospel of Jesus Christ ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"* "The projects of these vaccinators seem (it was affirmed) to bid bold defiance to heaven itself, even to the will of God."† "Providence (reasoned another author) never intended that the vaccine disease should affect the human race, else *why* had it not, before this time, visited the inhabitants of the globe. The law of God (he continues) prohibits the practice ; the law of man and the law of nature loudly exclaim against it."‡

Such historical facts and efforts, and the results in which they have invariably terminated, are surely sufficient to make men cautious and hesitating against always recklessly calling up again the same religious, or supposed religious, arguments under the same circumstances.§ Views and arguments of this description against every new practice intended to increase the well-being and happiness of mankind, certainly are greatly more calculated to inflict damage than benefit upon the interests of true religion.

Probably I may here be excused adding, that my friend Pro-

* Blair's Vaccine Contest, p. 84.

† Rowley on "Cow-pock Inoculation ; with the Modes of treating the Beastly new Diseases produced by it," p. 9.

‡ Dr. Squirrell's Preface to the second edition of his "Observations on Cow-pox, and the dreadful consequences of this new Disease," p. iv.

§ Perhaps, in the history of misplaced religious arguments against all novel opinions and practices, none in the retrospect may appear stranger than one that has been repeatedly mentioned to me during the few past months. Formerly, among my countrymen, most agricultural operations were performed, as commanded in the primeval curse, by personal exertion, and the "sweat of the face." Corn, in this way, was winnowed from the chaff by tossing it repeatedly up into the air, upon broad shovels, in order that any accidental currents which were present might carry off the lighter part. At last, however, about a century ago, "fanners," or machinery made for the production of *artificial* currents to effect the same purpose, were invented and introduced into different parts of the country. Some of the more rigid sects of Dissenters loudly declaimed against the employment of any such machinery. "Winds (they argued) were raised by God alone, and it was irreligious in man to attempt to raise wind for the aforesaid purpose for himself, and by efforts of his own." Mr. Gilfillan, the well-known Scottish poet, has furnished me with evidence of one clergyman debarring from the communion of the Lord's Supper those members of his flock who thus irreverently used the "Devil's wind" (as it was termed.) And such sentences, I believe, were not uncommon almost within the memory of some aged members of the present generation. Sir Walter Scott, in his *Old Mortality*, introduces honest Maule Headrigg as charging the Lady Margaret Bellenden and the authorities at Tillietudlem with abetting this reprehensible practice. "And since your leddyship is pleased to speak o' parting wi' us, I am free to tell you a piece o' my mind in another article. Your leddyship and the steward hae been pleased to propose that my son Cuddie suld work in the barn wi' a new-fangled machine for digging the corn frae the chaff, thus impiously thwarting the will of Divine Providence, by raising wind for your leddyship's ain particuliar use by human art, instead of soliciting it by prayer, or waiting patiently for whatever dispensation of wind Providence was pleased to send upon the sheeling hill." (Chap. vii.)

fessor Miller informs me, that when reluctantly consenting to write the elaborate article on Etherization, which he afterwards penned for the North British Review (No. for May 1847), he stated to the late Dr. Chalmers, who solicited him to undertake the task, that if he "wrote the medical, Dr. Chalmers should himself write the theological part." Dr. Chalmers at once professed that he did not see any theological part pertaining to it. Mr. Miller then explained to him, that some had been urging objections against the use of ether in midwifery, on the ground of its so far improperly enabling woman to avoid one part of the primeval curse. At last, when Mr. Miller was enabled to convince him that he was in earnest in saying that such ground *had* been taken, Dr. Chalmers thought quietly for a minute or two, and then added, that if some "small theologians" really took such an improper view of the subject, he would certainly advise Mr. Miller not to "heed them" in his article. Dr. Chalmers' mind was not one that could take up or harbour the extraordinary idea, that, under the Christian dispensation, the God of Mercy should wish for, and delight in, the sacrifice of woman's screams and sufferings in childbirth. Perhaps he thought also, as I have heard other clergymen state, that if God has beneficently vouchsafed to us a means of mitigating the agonies of childbirth, it is His evident intention that we should employ these means. The very fact that we have the power by human measures to relieve the maternal sufferings, is in itself a sufficient criterion that God would rather that these sufferings be relieved and removed. If He had willed and desired them not to be averted, it would not be possible for man to avert them. For while it is our duty to avoid all misery and suffering that is avoidable, it would certainly be impossible for us to eschew any that God had permanently and irreversibly decreed should not be eschewed.

7. I have heard objections urged against the state of anæsthesia as a counteraction to pain in surgery and midwifery, on other and different grounds from any I have yet noticed, viz., that in superinducing a temporary absence of *corporeal* sensibility, we also superinduce, at the same time, a temporary absence of *mental* consciousness. And it is argued, that, as medical men, we are not entitled to put the activity and consciousness of the mind of any patient in abeyance, for the mere purpose of saving that patient from any bodily pain or agony. Some medical men even, have gravely pressed this argument. But if there were any propriety in it, why, then, these same medical men could never have been justified in doing what they have one and all of them done perhaps hundreds of times; viz., exhibit, by the mouth, opium and other narcotics and hyp-

notices to their patients, to mitigate pain and superinduce anæsthesia and sleep. There is no greater impropriety or sin in producing sleep and freedom from pain by exhibiting a medicine by the mouth than by exhibiting it by the lungs. There is *less* impropriety in the latter practice than in the former, even according to the very doctrine of these opponents. For narcotic or anæsthetic agents which are swallowed, are far more prolonged in their "insensibilizing" action upon both the mind and body than those that are inhaled. The questionable character of the practice (supposing it for a moment to be questionable), must be much less when the effect is short and evanescent, as with ether and chloroform when respired, than when it is long and protracted, as with opium, morphia, henbane, &c., when swallowed. The proper anæsthetic state is one physiologically and psychically analogous to natural deep sleep. It is an artificial deep sleep. Those who object and urge that we should never follow ourselves, or induce others to follow, the practice of voluntarily surrendering up our mental consciousness for a time, in order to avoid any corporeal torture or agony that we would otherwise endure during that time, forget how often and how long they and others are in the habit of voluntarily surrendering up their mental consciousness in common sleep, far, far, beyond the time required merely for the refreshment and renovation of the system. Many thus *daily* surrender their minds and reason up for unnecessary hours to the state of unconsciousness existing in common or natural sleep, without any object except the reprehensible indulgence of sloth and indolence: and then they turn round, and declaim against others having induced upon them, at some *rare* and extraordinary time, the unconsciousness of artificial sleep, when there is a great and laudable object in view, viz., the avoidance of excruciating corporeal suffering, and the saving of human life by saving the human system from the shock and dangers accompanying that suffering.* Besides those that urge, on a kind of religious ground, that an artificial or anæsthetic state of unconsciousness should not be induced merely to save frail humanity from the miseries and tortures of bodily pain, forget that we have the greatest of all examples set before us for following out this very principle of practice. I allude to that most singular description of the preliminaries and details of the first surgical operation ever performed on man, which is contained in Genesis ii. 21:—"And the Lord God caused a deep sleep to fall upon Adam; and he

* See evidence of its saving human life, as well as saving human suffering, under surgical operations, in a table which I have given of the results of amputations with and without etherization, at p. 99 of "Remarks on the Superinduction of Anæsthesia in Natural and Morbid Parturition."

slept; and he took one of his ribs, and closed up the flesh instead thereof." In this remarkable verse the whole process of a surgical operation is briefly detailed. But the passage is principally striking, as affording evidence of our Creator himself using means to save poor human nature from the unnecessary endurance of physical pain. "It ought to be noted (observes Calvin in his commentary on this verse), that Adam was sunk into a profound sleep, in order that he might feel no pain."* In his collected commentaries on the same verse, Pool quotes different authorities for the same opinion, that this deep sleep was induced upon Adam in order that "he might not feel pain from the removal of the rib."† And the *profundity* of the sleep, as expressed in the Hebrew, is also worthy of note. For the noun "*tardemah*," translated in our version "deep sleep,"‡ signifies, according to all the best Hebrew scholars, the deepest form of induced slumber. In the early and very literal Greek translation which Aquila made of the Bible, he renders, in this passage, the Hebrew word *tardemah* by the expressive Greek term *καταφορά*, a term which Hippocrates, Galen, Ætius, and other Greek physicians, used as implying that state of insensibility and total unconsciousness which in modern medical language we express by "coma" and "lethargy."§ Gesenius renders *tardemah* by the Latin word "sopor," the Hebrew term for common sleep being *shenah*. In the Vulgate it is translated "sopor" (*immisit Deus soporem in Adam*). In the quotation which I have given from Calvin, that great authority renders the term *tardemah* by the expression profound "sopor" (*profundo sopore*); and Pool quotes different authorities to show that the Hebrew word does signify "sopor" of a profound kind, "*notat profundum soporem*."||

* "Notandum, Adam profundo sopore fuisse demersum, ut nihil doloris sentiret."—*Johannis Calvini in Librum Geneseos Commentarius* (Hengstenberg's Edition, page 36).

† "Ne ablationis costæ dolorem sentiret."—*Poli Synopsis Criticorum aliorum-que Scripturæ Interpretum*. Vol. i. p. 29.—See also the same opinion expressed in Rosenmüller's *Scholia Vetus in Testamentum*, vol. i. p. 106, "Adamo, somno sopito, ne dolorem sentiret;" and in the English Commentaries of Bishop Patrick, p. 14, "Whereby he was made less sensible of the pain which otherwise he would have felt in the opening his side;" and of Drs. O'Doyle and Mant, "Adam was thus less sensible of bodily pain;" &c., &c.

‡ In Luther's German Bible, an exactly corresponding expression "*tiefen schlaf*" is used. In Dathe's valued Latin version of the Pentateuch, a similar translation is given, "*Deus gravem Adam soporem immisit*," p. 27.

§ "Cataphora (from *καταφέρω* to sink or fall down,) a term used by some authors to designate a state of conia, and by others an unusually profound sleep."—*Hooper's Medical Dictionary*.

|| See his *Synopsis Criticorum et Scripturæ Interpretum*, p. 29.

CHAPTER IV.

SAME SUBJECT CONTINUED, IN A LETTER TO DR. PROTHEROE SMITH,
OF LONDON.

Edinburgh, 8th July, 1848.

MY DEAR SIR,—

According to promise, I sit down to write you a few hurried notes on the subject of the avowed religious objections to the adoption of Anæsthesia in Human Parturition.

I regret to hear from you that, in London, the progress of Anæsthetic Midwifery is impeded by any groundless allegations as to its unscriptural character; and I can sincerely sympathize with you in your exertions to annihilate these scruples. Here, in Edinburgh, I never now meet with any objections on this point, for the religious, like the other forms of opposition to chloroform, have ceased among us.

But in Edinburgh matters were very different at first. I found many patients with strong religious scruples on the propriety of the practice. Some consulted their clergyman. One day, on meeting the Rev. Dr. H——, he stopped me to say that he was just returning from absolving a patient's conscience on the subject, for she had taken chloroform during labour (and so avoided suffering), but she had felt unhappy ever since, under the idea that she had done something very wrong and very sinful. A few among the clergy themselves, for a time, joined in the cry against the new practice. I have just looked up a letter which a clergyman wrote to a medical friend, in which he declares that chloroform is (I quote his own words) "a decoy of Satan, apparently offering itself to bless woman; but, in the end (he continues), it will harden society, and rob God of the deep earnest cries which arise in time of trouble for help." And you are aware how earnestly some medical men attempted to preach, and, as you state, still preach against it on religious grounds. The medical friend who sent me the note from which I have quoted, himself read a wild and fanatical paper before the Medical Society of — on the subject; and, I am told, it met with no small favour from the Society. I have enclosed a copy of this paper for your

perusal. Some Lecturers on Midwifery, in London and Dublin, publicly adopted the same line of opposition and argument.

With the view of meeting, if possible, these strange and extraordinary objections, I wrote, in December last, a pamphlet on the subject of the so-called Religious Reasons against the employment of Anæsthetic Agents in Midwifery and Surgery. After its publication, I received a variety of written and verbal communications from some of the best theologians and most esteemed clergymen here and elsewhere, and of all churches, Presbyterian, Independent, Episcopalian, &c., approving of the views which I had taken. I have letters of the same kind from some men of high rank in your church; and a note in approval was brought to me, emanating from one of your most exalted and most esteemed episcopal dignitaries.

The pamphlet itself, however, was no doubt imperfect. It was principally written in a day's confinement to my room when convalescing from the prevailing influenza. I do not know what views you intend to take in your forthcoming publication, but there are some points on which, if I had had time, I would perhaps have more insisted on in mine; and, if you will bear with me, I will briefly state them.

1. In the whole inquiry nothing appeared to me more satisfactory or striking than the philosophic precision of the language of the Bible upon the point; and I did not sufficiently insist upon this, as an evidence of the fact that the primal curse on woman did *not* refer to the pure physical sufferings and agonies of parturition. Each so-called labour-pain consists, as you well know, of two distinct and separate elements; viz. *first*, of contraction of the uterus and other assistant muscles; and, *secondly*, of sensations of pain, more or less agonizing, accompanying these contractions, and directly resulting from them. Now, I have been often struck, as you must have been, in chloroform labours, with the fact that, in the anæsthetic state, not only does the uterus contract powerfully, but that the abdominal muscles often do so also, and even the face of the patient will sometimes betoken strong expulsive muscular action, while all accompanying suffering is quite annulled. We abrogate the second element of the so-called labour pain, without destroying the first. We leave intact the expulsive muscular efforts, but remove the sense and feeling of pain accompanying these efforts. It is only of late that these two elements or constituents of labour-pains have been recognised and studied by the Profession as *two* separate objects. But it is surely, as I have above stated, worthy of remark and wonder, that the language of the Bible is, on this as on other points, strictly and scientifically correct, and long ago made,

with perfect precision, the very distinction which we are now-a-days only recognising. For the Hebrew noun, *'etzebh*, distinctly signifies the muscular contraction or effort, and the nouns, *hhil* and *hhebbhel*, as distinctly signify the sensations of pain accompanying these efforts; and as you are aware (as I have elsewhere fully shown), it is not the latter but the former of these nouns that is used in the language of the primary curse:—"In sorrow (*'etzebh*) shalt thou bring forth." Now, I repeat, the efforts or muscular contractions (the *'etzebh* of the curse) are, as I have just stated, left in their full and complete integrity under the state of anæsthesia; while the pangs or sufferings (or *hhil*), against which the language of the curse does *not* bear, are alone annulled and abrogated.

2. Some of your London medical divines, however, argue, I hear, that *'etzebh* must mean pain,—and that, as meaning such the curse *must* be taken *literally*; and hence that woman *must* be allowed to go on suffering. In the pamphlet referred to, I have attempted to answer this by showing that then we, of the sex of Adam, must adhere literally also to the words of the curse, as far as they apply to us, and hence must earn our bread by the "sweat of our face," and by that only. Nay, the very physicians who thus insist on reading and acting upon this and other texts literally—and literally only—forget, I fear, that (according to their own doctrines) in practising physic, they are really and truly practising a profession of sin and iniquity, in so far as man in the primeval curse was doomed to die, and yet they daily and hourly persist in attempting to make him live. An esteemed clerical friend, in writing to me on the matter, stated that he was afraid *his* cloth was perhaps even more sinful than ours—if this outrageous view were true;—for the introduction of sin was the consequence of the fall, and the church, in labouring to banish and abrogate that effect—in trying to turn mankind from sin—were actually trying to cancel the greatest and most undoubted effects of the first curse upon the human race.

3. But the accoucheurs and surgeons among you who object to the use of chloroform, on the ground that it goes, in their opinion, against the object and end of the primeval curse upon woman, strangely forget that the whole science and whole art and practice of midwifery is, in its essence and object, one continuous effort to mitigate and remove the effects of that curse. By warm baths, aperients, regulated diet, &c., they attempt to destroy the intensity of the approaching pains and penalties of childbirth;—during labour, they use counter-pressure on the back, to relieve the intense pains there; they use unguents, perineal support, venesection, &c., &c., to ease the pains and

insure the safety of the mother. By these means they succeeded partially, in times past, in mitigating the sufferings and effects of parturition, and thought they committed no sin. But a means is discovered by which the sufferings of the mother may be relieved far more effectually; and then they immediately denounce this higher amount of relief as a high sin. Gaining your end, according to their religious views, imperfectly, was no sin—gaining your end more fully and perfectly is, they argue, an undiluted and unmitigated piece of iniquity. To relieve our patients, however, by our interference, a little, and a little only, is assuredly, in a moral and religious point of view, just as sinful as if we succeeded in affording them complete relief from suffering. The *principle* of interference is not altered by the *degree* of relief afforded being more or less, greater or smaller. “For whosoever shall keep the whole law, and yet offend in one point, he is guilty of all.” If, on religious grounds, your obstetric friends object to relieving entirely a woman of her worst pains (now that they have the means of doing so), they must, on the very same grounds, refuse to relieve her imperfectly and partially of these or any other pains and sorrows connected with parturition; they must, or at least ought to abstain, in fact, from all obstetric practices whatsoever; they should, in short, give up their present profession, as a profession of sin—and “in the sweat of their face” eat bread. I can see no other possible alternative for them, provided (that is to say) they choose to reduce actually their theory into practice. If, on the other hand, they think it not sinful to relieve their female patients, to a small amount, from the alleged sufferings entailed upon them by the first curse, then surely it is not sinful in them to relieve their patients from their sufferings to a far greater amount, now that they have the power of doing so—nay, is it not sinful in them obstinately to withhold that relief? For, “to him that knoweth to do good, and doeth it not, to him it is *sin*.”

These remarks apply to medical practitioners. And if any of your female patients hold the same groundless doctrine—(a doctrine far more in accordance with the blindness and fatalism of Mahomedanism, than with the spirit and genius of Christianity)—if they hold that it is improper, for scriptural reasons, to abrogate the pains and sufferings of childbirth, then such mothers cannot conscientiously content themselves with rejecting merely the use of chloroform in annulling the pangs of parturition; they must reject all kind of medical assistance in their hour of travail; they must give up, indeed, all assistance whatever. If the supposed pains and perils of the primeval curse are to be submitted to, on the ground that they are divinely

appointed, and unavoidable ordeals,—then they must be submitted to in all their unmitigated power and plenitude; no doctor must sinfully dare to stay the ebbing stream of life, if a fatal flooding suddenly supervene during labour; no nurse must venture, as heretofore, to relieve and mitigate the agonies of the shrieking mother by counter-pressure to her back, &c., “for whosoever shall keep the whole law, and yet offend in one point, he (she) is guilty of all.”

4. Those who object to the adoption of anæsthesia in midwifery, on religious grounds, entirely forget, that, if God had really willed the pains of labour to be irremovable, no possible device of man could ever have removed them. I have elsewhere attempted to state this argument, but it is so much better and more clearly given in a letter in my possession, from the pen of a clergyman, Dr. ———, who is acknowledged to be one of the ripest biblical scholars, and most profound theological critics and writers of the present day, that I will cite his letter at length to you. I received it a few days after the publication of the pamphlet I have referred to.

“I have just finished the perusal of your pamphlet, entitled, *Answer to the Religious Objections advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery*; and I cannot refrain from expressing to you, though almost a stranger to you, the gratification which I have derived from it. I think your argument irrefragable, both as respects the question of philology and as respects the moral question; and, as a theologian, I feel very grateful to you for so ably wiping away the reproach from the Bible, of discouraging any attempt to mitigate the sufferings of mankind. I am very sure the word of God, the revelation of His love and grace to man, has no such aspect;—and that it is only injudicious and ignorant zeal that leads any of its professed disciples to speak as if it had.

“The objection which you so ably expose is not, as you observe, novel—though now, for the first time, adduced in the special application of it to the relieving of the pains of childbirth. I remember when many pious people had great scruples about endeavouring to emancipate the negroes, on the ground that they were the descendants of Ham, on whom the curse of perpetual slavery had been pronounced. I should not be surprised, in the course of the debates upon the emancipation of the Jews, to find some members pleading, as some have pleaded in former times, that to give a Jew a legitimization in any commonwealth is a plain contravention of the will and word of God concerning that people.

“It has strongly appeared to me, for many years, that there are two principles laid down in Scripture, a due regard to

which would preserve good people from those hasty applications of Scripture predictions—whether minatory or otherwise. The one is the Apostle Peter's course for the understanding of predictions, that "no prophecy is self-interpreting (*ἰδίᾳ ἐπιλυσέως*)," from which it follows that in the case of a prediction threatening we are to get at its meaning not from the words themselves in which it is couched as from those in connexion with the events or circumstances by which the Almighty and Allwise fulfils his own declarations. The other is the obvious truth that God's blessing and God's curse no one can reverse; so that if any class enjoying God's blessing meet with pains, or any class exposed to his curse enjoy relief or advantage, the first inference is that the pain was not excluded by the blessing, nor the benefit by the curse. Applying these principles to the case you have so ably discussed, I came speedily to the conclusion that as you could not by chloroform, or any thing else, set aside God's curse, and as the primary threatening, is like all predictions to be interpreted by events in God's providence, the mere fact that by the adhibition of that agent you could relieve women from the agonies of childbirth, was to me proof sufficient that these mere agonies were not designed to form any essential part of that curse. The justice of this conclusion *a priori* your pamphlet amply substantiates by inductive reasoning."

5. The employment of anæsthesia in obstetric and medical practice is in strict consonance with the whole glorious spirit and beneficent arrangements of the Christian dispensation—for, all our greatest divines are agreed, I believe, on one point, viz., that this dispensation, in the application of its principles and precepts, is intended and calculated not only to regenerate and advance our moral condition, but more and more to ameliorate the physical sufferings and state of mankind. Witness, for example, the mighty power and resistless influence by which it has gradually acted, and is acting (through the development of its rules and doctrines) in the extermination from this earth of the curse of human slavery.

6. Some thoughtlessly argue that the employment of anæsthetic means and the abrogation of pain in labour must be irreligious, because it is "unnatural." They seem to think that it looks as if we fancied that nature, or rather that the God of Nature had made the function of parturition in some respects imperfect or improper in its mechanism. These same individuals strangely forget that they themselves do not think it "unnatural" to assist and supplement other physiological functions of the body. They wear clothes to assist the protecting influence of the skin, and do not think that "unnatural." They

use cookery and condiments to aid the functions of mastication and digestion. Is this because they think that nature has left the functions of mastication and digestion imperfect in their formation or mechanism? They constantly ride in coaches, &c. Is the function of progression imperfect in man? "How unnatural," exclaimed an Irish lady to me lately, "how unnatural it is for you doctors in Edinburgh to take away the pains of your patients when in labour." "How unnatural (said I) is it for you to have swam over from Ireland to Scotland against wind and tide in a steamboat." Many habits and practices,—in fact, almost all the habits and practices of civilized life are really and fundamentally as "unnatural" as assisting the function of parturition by inducing anæsthesia during it. But we do not look upon them as such, simply because they are already passed into acknowledged and universal adoption. Those who lived at the time when each separate infringement and improvement took place, could perhaps tell a story of doubt and opposition not unlike that which we can now do with regard to anæsthetic midwifery. And those who have taken up this ground of opposition, in the present as in other cases, always seem, for the time being, to forget that it is God who has endowed man with mental powers calculated gradually to enable him to extend his knowledge and improve his earthly condition, and that this extension and this improvement are so far evidently allowed and willed by God himself.

7. An additional argument is suggested in a letter lying before me, from a gentleman of high name in the literary world, to his son, who was a pupil of mine during the last winter. To understand its applicability, however, let me premise one or two words.

Some months ago, I published an extensive and carefully collected series of statistical returns from various British hospitals, showing the operations of surgery were much less fatal in their results, when patients were operated on under the condition of anæsthesia, and consequently without any attendant suffering, than when, formerly, they were submitted to all the horrors and agonies of the surgeon's knife in their usual waking and sensitive state. Thus, I found that, while, before the introduction of anæsthesia, in every 100 cases of amputation of the thigh performed in our hospitals, from 40 to 45 of the patients died; the same amputation when performed upon anæsthetized patients, did not prove fatal to more than 25 in the 100 cases; or in other words, out of every 100 such operations, the previous induction of anæsthesia was the means of preserving 15 or 20 human lives. So much are all our surgeons here impressed with the fact that the state of anæsthesia saves their

patients alike from pain, and from the subsequent hazards and dangers of pain, that I believe not one among them would deem himself justified in submitting a fellow-being to the tortures of the operating table, without the previous employment of chloroform. And, I believe, you are aware that we also use it here, in Edinburgh, constantly in midwifery—its omission being the exception, and a rare exception, to the general rule of its employment. By thus shielding our patients against the more severe portion of the pains of parturition, we not only save them from much immediate suffering, but we save their constitutions also from the effects and consequences of that suffering; and, as a general rule, they assuredly make both more rapid and more perfect recoveries. I most sincerely believe, that, in thus cancelling the pains of labour, we also, to a great extent, cancel the perils of it; for all our highest authorities in pathology, admit that pain, when either great in excess, or great in duration, is in itself, and by itself, deleterious and destructive: and the mortality accompanying parturition is regulated principally by the law of the length and degree of the patient's struggles and sufferings. In the Dublin Lying-in Hospital, when under Dr. Collins's able care, out of all the women, 7050 in number, who were delivered within *two* hours from the commencement of labour, 22 died; or *one in every 320*. In 452 of his cases, the labour was prolonged above *twenty* hours, and out of these 452 mothers, 42 died; or *one in every 11*; a difference enormous in amount, and one strongly calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

Now the writer of the letter to which I have alluded, is the author of one of the most eloquent essays in the English language, on the holy character and genuineness of the Bible. He is not a physician, though deeply read in medical, as in all other forms of knowledge; and, aware of the dangerous and destructive properties of severe pain, when unmitigated and unrestrained, he reasons thus; "If pain when carried—as in parturition—to the stage which we call agony, or intense struggle amongst the vital functions, brings with it some danger to life, as I presume no one can deny *must* be the case, then it will follow, that, knowingly to reject a means of mitigating, or wholly cancelling the attendant suffering and its dangers, (now that such a means has been discovered,) travels, in my opinion, on the road towards suicide. If I am right in believing that danger to life lies in this direction, then, clearly, the act of rejecting the remedy against it, being wilful, lies in a suicidal direction. It is even worse than an ordinary movement in that direction, because it affects to make God an ac-

complice through the Scriptures in this suicidal movement, nay, the primal instigator to it, by means of a supposed curse interdicting the use of any means whatever, though revealed by Himself for annulling that curse." The same argument which is here brought against the wilful rejection of anæsthetic measures by the patient, necessarily applies with the same spirit, but with some changes in the terms, against the wilful rejection of the same means by the medical attendant.

But I must be done; for I fear I have exhausted your patience as well as my own time. Let me merely add that I am sure you deeply regret and grieve with me that the interests of genuine religion should ever and anon be endangered and damaged by weak but well-meaning men believing and urging that this or that new improvement in medical knowledge, or in general science, is against the words or spirit of Scripture. We may always rest fully and perfectly assured that whatever is true in point of fact, or humane and merciful in point of practice, will find no condemnation in the Word of God.

With many apologies for the unexpected length to which these remarks have extended,

Believe me, my dear Sir,

Very faithfully yours.

J. Y. SIMPSON.

CHAPTER V.

ON THE EARLY HISTORY AND PROGRESS OF ANÆSTHETIC MIDWIFERY.

———"I do think you might spare her.
And neither heaven nor man grieve at the mercy."
Measure for Measure.

IN a communication laid before the Edinburgh Medico-Chirurgical Society in November last, I attempted to prove that the idea of cancelling and abrogating the pains inflicted by the knife of the surgeon had not entirely originated in our own times. I showed that Dioscorides, Pliny, Apuleius, Theodoric, Paré, and others,* had long ago described, and some of them apparently practised, the induction of anæsthesia, previous to operations, both by giving their patients narcotic substances to swallow and narcotic vapours to inhale. While making the researches upon which the communication alluded to was founded, I further attempted to ascertain if any writer had proposed to assuage or annul, by the same or by other means, the pains attendant upon human parturition. I failed, however, in finding any traces whatever either of any practical attempts to abrogate or modify, by true anæsthetic means, the pains of labour, or of any theoretical suggestions even as to the very possibility of effecting that desirable result. And I believe the history of the induction of anæsthesia in midwifery does not date far back, like the history of anæsthesia in surgery. The first instance in which the practice was adopted, occurred in my own practice in Edinburgh on the 19th January, 1847. The case was one of deformed pelvis, in which I had predetermined to extract the child by turning, and to try the inhalation of ether vapour upon the mother, with a view to facilitate that operation. During a week or two previously, I had anxiously waited for the supervention of labour in this patient; for, by the result, I expected that much would be decided in regard to the effect of ether-inhalation in parturition. Would it merely avert and abrogate the sufferings of the mother, without interfering with the uterine contractions? Or, would it arrest si-

* Monthly Journal of Medical Science, vol. 1847-48, p. 451.

multaneously both the contractions of the uterus and the sufferings that arise from them? As far as the proposed mode of delivery by turning was concerned, it was a matter of no vital importance whether the etherization stopped the uterine contractions or not. And, on this circumstance, depended the eligibility of the case for a first trial of ether-inhalation. The result was most satisfactory and most important; for it, at once, afforded me evidence of the one great fact upon which the whole practice of anæsthesia in midwifery is founded—it proved, namely, that though the *physical sufferings* of the parturient patient could be annulled by the employment of ether-inhalation, yet the *muscular contractions* of the uterus were not necessarily interfered with; or, in other words, that the labour might go on in its course, although the sensations of pain usually attendant upon it were, for the time being, altogether abrogated.

This case, with its more obvious results and inferences, was communicated to the Obstetric Society at their meeting on the 20th of January.* In the course of the subsequent three weeks I had an opportunity of trying ether-inhalation in several cases of natural labour and in one forceps case; and, at the next meeting of the Obstetric Society on the 10th February, I took an opportunity of bringing the subject under the attention of the members at greater length. In the published reports of the Society,† the various inferences which then appeared to me to be deducible in regard to it are given in the following terms:—

1. That the inhalation of ether procured for the patient a more or less perfect immunity from the conscious pain and suffering attendant upon labour.

2. That it did not, however, diminish the strength or regularity of the contractions of the uterus.

3. That, on the other hand, it apparently (more especially when combined with ergot) sometimes increased them in severity and number.

4. That the contraction of the uterus after delivery seemed perfect and healthy when it was administered.

5. That the reflex assistant contractions of the abdominal muscles, &c., were apparently more easily called into action by artificial irritation, and pressure on the vagina, &c., when the patient was in an etherized state.

6. That its employment might not only save the mother from more pain in the last stage of labour, but might probably save her also, in some degree, from the occurrence and consequences of the nervous shock attendant upon delivery, and thereby reduce the danger and fatality of childbed; and,

7. Its exhibition did not seem to be injurious to the child.

Full details of some of the principal cases upon which these inferences were founded, were, along with other additional instances, subsequently thrown together and published in the form of a communication to the *Monthly Journal of Medical*

* Monthly Journal, vol. 1846-47, p. 639.

† Ibid. p. 795.

*Science.** In that paper I made some observations on the question, Whether it would be proper to employ anæsthesia in natural labour? I adduced various reasons from physiology and pathology for believing, that the parturient action of the uterus would go on healthily and uninterruptedly though the influence of the mind and purely cerebral functions were suspended, and that the dangers of the nervous shock attendant upon labour would be lessened; and I pointed out the necessity of ascertaining, by a cautious series of observations, what counter-indications there might be to the employment of the practice;—whether it were ever apt to give a tendency to hemorrhage or other complications; its influence, if any, upon the child; the length of time its use might be continued in any one case, &c.

At the date at which the paper that I have just referred to was written, viz., the 18th February, the longest time during which I had ventured to keep a parturient patient in the anæsthetic state was about half an hour. And many who believed that this state might be induced without danger for a few minutes, entirely doubted whether it could be sustained for any great length of time without extreme hazard. During the experience, however, of the next two or three weeks, I ascertained the fact, that the anæsthetic action could be safely kept up during labour for one, two, three, or more hours. At one of the subsequent meetings of the Obstetric Society, this result and others were adduced,† and the following additional deductions drawn, as stated in the words of the printed proceedings of the Society, viz.—that,

1. The state of etherization had little or no influence upon the fetus, none, at least, of a deleterious kind,—the fetal heart increasing only a few beats, if at all, when the mother was kept long and fully etherized, either during pregnancy or labour.

2. The mother, during labour, may be kept etherized, if required, for one, two, three, or more hours. Dr. Simpson described two cases, in one of which the mother was about six, and in the other, about four hours etherized before the children were born. In both cases the duration of the intervals and of the pains before and during etherization was noted (as in the experiments which Dr. Simpson had published on galvanism), and the etherization seemed to have no effect either on their frequency or strength. But,

3. In two or three cases, Dr. Simpson had seen a very deep state of etherization modify apparently the full strength of uterine contractions, but they recurred immediately in full force when the patient was allowed to fall back into a state of slighter etherization.

4. Dr. Simpson had hitherto seen no traceable injury to either mother or child from its employment, but the reverse.

5. The inhaler he used was either a concave sponge saturated interiorly with ether and held over the face, or a simple portable flask without valves.

* "Notes on the Employment of the Inhalation of Sulphuric Ether in the Practice of Midwifery."—Monthly Journal of Medical Science for March, p. 721.

† Monthly Journal of Medical Science, 1847-48, p. 214.

The first case of labour in which I employed artificial anæsthesia occurred, as I have already stated, on the 19th January, 1847. This case and its results were stated publicly, on the following day, to my class in the university, and immediately became extensively known to the profession through the medium of the public journals.* In the course of a short time the practice of anæsthesia began to be tried in other medical schools. On the 13th February, Dr. Murphy of London stated to the Westminster Medical Society that he had employed it in a case of turning.† On March 27th a case was published of the use of ether in natural labour,‡ by Mr. Lloyd. Dr. Protheroe Smith delivered a patient under a state of anæsthesia on the 28th of March; and in the *Lancet* for 1st May he published a paper, "On the Employment of Ether by Inhalation in Obstetric Practice," giving an account of this and two later cases in which he had recourse to anæsthesia during labour. He afterwards sent to the same journal several additional cases and remarks.§ Mr. Lansdowne of the General Hospital, Bristol, subsequently published various cases in which the practice was successfully employed. His first case occurred on the 8th April.||

In Ireland, the first case delivered in a state of artificial anæsthesia was on the 28th November, 1847. The patient was under the care of Dr. Tyler of Dublin. It was an instrumental labour.

In France, the practice was much more early tried. In about a week after the first case occurred in Edinburgh, Fournier Deschamps delivered a patient by the forceps when she was in a state of anæsthesia.¶ This was on the 27th of January. On the 8th of February, Professor Paul Dubois exhibited ether in a case of forceps operation at the Hôpital de la Maternité of Paris, and up to the 23d February had used it in four other deliveries. He has not himself published, as far as I am aware, any written observations of his own upon the subject; but he early brought the question, in an interesting and able form, before the French Academy of Medicine,** and

* See Medical Gazette for 1847, vol. xxxix. p. 460. Also Provincial Journal for 1847, p. 84.

† *Lancet* for February 27, 1847.—Before this date, viz., on the 26th of January, a state of anæsthesia was attempted to be induced in a patient upon whom the Cæsarean section was performed by Mr. Skey of St. Bartholomew's Hospital, London. "But the inhalation of the vapour of ether was unsuccessful, or but very partially successful."—(*Lancet*, vol. i. 1847, p. 140.)

‡ Medical Times, 1847, p. 96.

§ *Lancet* for 1847, vol. ii. p. 121, and p. 305.

|| *Lancet*, vol. i. 1847, p. 446.

¶ *Gazette des Hôpitaux*, 30 Janvier, 1847.

** *Bulletin de l'Acad. Roy. de Méd.*, tom, xii. p. 400.

various reports of this important oral communication immediately appeared in different journals.*

Professor Dubois' conclusions, as officially reported in the *Bulletin de l'Académie*, were as follows:—†

1. The inhalation of ether can annul the pain of obstetrical operations.
2. It can suspend the physiological pains of labour.
3. It destroys neither the uterine contractions, nor the contractions of the abdominal muscles.
4. It diminishes the natural resistance of the perineum.
5. It does not appear to act unfavourably on the health or life of the infant.

Professors Villeneuve‡ of Marseilles, and Stoltz§ of Strasbourg, subsequently published some additional cases and observations.

In Germany the first case of anæsthetic midwifery, of which I have found any published notice, occurred on the 24th February, 1847, under the care of Professor Martin of Jena.¶ The ether was administered during the operation of separating and extracting an adherent placenta. Professor Siebold¶ read a paper on the subject before the Royal Scientific Association of Goettingen on May 8. He had employed ether in several cases of natural and instrumental labour. On the 3d June, Professor Grenser of Leipsic** etherized a patient for a forceps operation, and afterwards resorted to it in several other natural and instrumental cases.

In America,—the country to which we are indebted for the first knowledge of the anæsthetic effects of sulphuric ether in surgical operations,—the same agent was not employed in midwifery till the reports of its use in obstetric practice in Europe had recrossed the Atlantic. Dr. Channing, professor of midwifery in Harvard University, was, I believe, the first to employ it in labour.†† He adopted the practice in two forceps cases; the first of which occurred on May 5, the other on May 15, 1847. Drs. Clark,‡‡ Putnam,§§ and other American practi-

* "Le 23 Février, M. P. Dubois, qui avait eu connaissance des recherches de M. Simpson, communiqua à l'Académie de Médecine le résultat de ses expériences." —(*Chambert. Des Effets des Ethers*, p. 231.)

† *Bulletin*, tom. xii. p. 407.

‡ De l'Éthérisation dans les Accouchements. Marseille, 1847.

§ *Gazette Médicale de Strasbourg*, 27 Mars, 1847.

¶ Ueber die Künstliche Anæsthesie bei Geburten. Jena, 1848.

¶ *Medical Gazette*, vol. xxxix. 1847, p. 1052.

** Ueber Aether-einathmungen während der Geburt. Leipzig, 1847.

†† Two cases of the inhalation of ether in instrumental labour. Boston, 1847. —(From the *Boston Medical Journal*.) Dr. Channing, in a postscript to and American reprint of one of my *Essays on Chloroform*, has announced his intention of soon publishing a volume "On the Employment of Etherization in Child-birth."

‡‡ *Philadelphia Medical Examiner*, March, 1848.

§§ *Boston Medical and Surgical Journal*, February 2, 1848.

tioners, have latterly published the results of their experience in anæsthetic midwifery.

In November, 1847, a new impulse was given to the practice of anæsthesia in midwifery, by the introduction of chloroform as a substitute for sulphuric ether. The ether required to be exhibited in large quantities in order to keep up its action; and hence it was objectionable in ordinary obstetric practice from its bulk, and the inconvenience of its carriage. Most medical men believed that an apparatus of more or less considerable size was necessary for its proper and effectual exhibition, thus further encumbering the practitioner. These and other difficulties were found not to appertain to the use of chloroform, and many, in consequence, adopted it in midwifery, who had previously altogether rejected the employment of sulphuric ether. The first case of labour in which I employed chloroform occurred on the 8th of November. On the 1st December, I reported to the Edinburgh Medico-Chirurgical Society a series of cases illustrative of its effects and use in natural and operative labours.* In the London weekly journals, since that period, Dr. Protheroe Smith, Dr. Murphy,† Dr. Rigby, Mr. Lansdowne, Mr. Brown, Dr. Bennet, Mr. Phillips, and others, have published cases and reports upon the subject.

* See *Lancet* for November 20, 1847, p. 533, and December 11, p. 613; also *Medical Gazette* for November 26, p. 934.

† See also Dr. Murphy's able Harveian Oration on the employment of Chloroform.

CHAPTER VI.

RESULTS OF THE PRACTICE OF ANÆSTHESIA IN MIDWIFERY.

SINCE January, 1847, up to the present time, I have, in my own practice, delivered about 150 patients under a state of anæsthesia. The results to the children and mothers have been as follows:—

Results to the Children.—In the 150 cases, all the children were born alive except one. In this exceptional instance, the infant was expelled in a decomposed and putrid state, between the seventh and eighth month of utero-gestation. It had not been felt to move, nor had I been able to hear the fœtal heart with the stethoscope, for two or three weeks previously. The mother had, before the present pregnancy, borne several premature dead children. Though the infant was small, yet the suffering attending upon its expulsion offered to be excessive, and, to relieve the mother of this unnecessary agony, I placed her under the influence of chloroform.

During the few weeks of my obstetric attendance after delivery upon these 150 cases, only one of the children died; namely, a child who sank under the symptoms of cyanosis. Nor am I aware that any of them has suffered, up to this time, under “cerebral effusions,” or “convulsions,” or “hydrocephalus,” or any other of the affections which have been prophesied as certain to befall all such infants as would be borne in labours rendered painless by art. Perhaps it may be proper to add, that none of the children have shown any symptoms of what has been calmly averred, in more than one publication in London, as a possible, or rather probable result of anæsthesia, viz. idiocy.*—“Dixerit insanum qui te, totidem audiet.”

* How can we “know or ascertain the possible consequences of the use of such an agent on the brain of the child? And how can we calculate what may be the ultimate consequences of its action in reference to the development of the mental faculties?”—*Dr. Malan, in Lancet for April 29, 1848.*

“It is admitted by all, that the pulsations of the fœtal heart are *greatly increased* during inhalation,—indeed, to such an extent has this been noticed, that in some instances the pulsations could not be counted, so much were they accelerated. Are not effusions to be feared from this? Are not convulsions after birth likely

Results to the Mothers.—Among the 150 mothers, the more immediate and direct effect of anæsthesia has been the alleviation or abolition of the physical sufferings attendant upon the latter stages of labour. And, certainly, if the object of the medical practitioner is really twofold, as it has always, till of late, been declared to be, viz., “the alleviation of human suffering and the preservation of human life,” then it is our duty as well as our privilege to use all legitimate means to mitigate and remove the physical sufferings of the mother during parturition. The degree of these sufferings is, as a general rule, assuredly such as to call for this aid and mitigation. In proof of their severity, I might cite the unprejudiced testimony of various obstetric authors. At present, I shall content myself with one. Professor Meigs of Philadelphia—a declared opponent of the innovation of artificial anæsthesia in labour—when speaking of the sufferings of the mother in human parturition, fully admits their intensity. “*What* (says he) do you call the pain of parturition? There is no name for it but *Agony* ;” * and he elsewhere speaks of the pains in the last stage “as absolutely indescribable, and comparable to no other pains.” † Now, surely, if it be the duty of the physician (and who doubts it?) to relieve and remove the pains of colic, of pleurodyne, of headache, neuralgia, rheumatism, &c., &c., it is his duty to relieve pains so severe as to be “absolutely indescribable, and comparable to no other pains.” There is not one code of humanity for one class of pains and patients, and a different and opposite code for another class of them.

From November last, when I began to use chloroform in labour, up to the present time, none of the patients, with one exception, at whose delivery I have attended, has been aware of these last “absolutely indescribable” pains; the state of artificial anæsthesia having always been induced for a longer or shorter time before their supervention. And I have kept up

to ensue? And may not that occur which would make the most heartless mother shudder at the bare possibility of herself, by her want of courage, being instrumental in producing? May not *Idiotcy* supervene? Of this we have as yet no experience, nor shall we have, perhaps, for years; but when *one* such case occurs, will there then be found any one who will afterwards be persuaded to submit herself to etherization during pregnancy?”—*Mr. Gream, in London Medical Gazette for 7th September, 1848.*—It is perhaps superfluous to add, that the premises of the preceding paragraph are as gratuitous as its conclusions; and that the pulsations of the fetal heart are little, if at all, increased in rapidity when the mother is anæsthetized. “The action of the child’s heart (says Professor Siebold) was found to continue quite unaltered, not the slightest change in its frequency and regularity being detected.”—*Siebold on the Employment of Etherial Inhalations in Midwifery, in the Medical Gazette for 11th June, 1847.*

* Females and their Diseases, p. 49. I leave the italics the same as in the original.

† Philadelphia Practice of Midwifery, p. 153.

this state for a period varying from a few minutes to four, five, six, or more hours before delivery. In the exceptional case referred to, the patient's sufferings were greatly mitigated; but the state of anæsthesia was not, as usual, perfect and complete, the patient having been unexpectedly taken in labour when not in her own house, and the attendant anxiety and confusion of herself and her attendants being such as totally to preclude the requisite degree of quietude. When employing ether, I repeatedly saw cases in which the patients were thus only partially and not completely anæsthetized,—where, in other words, they were not entirely asleep, but were aware of the presence of the uterine contractions, and sometimes experienced, from them, sensations in some degree painful, but of a very mitigated and blunted character.

Besides thus alleviating and abolishing the sufferings of the mother during labour, the practice of anæsthesia carries along with it other advantages. A number of patients have spontaneously told me, that the prospect of being enabled to pass through the ordeal of parturition with the assistance of anæsthetic agents, and without their usual painful agonies, has destroyed, in a great measure, that state of anxiety and dread of anticipation, which, in former pregnancies, had, for weeks and months previously, silently annoyed and haunted them. If we can thus add to the happiness of our patients, by imparting to them feelings of safety and immunity under one of the severest trials to which nature exposes them, we surely follow out, in its truest sense, that which Dr. Meigs correctly describes the office of a physician to be,—namely, “a great mission of benevolence and utility.”

But the practice of anæsthesia in midwifery not only saves the mother from the endurance of unnecessary mental anxiety and unnecessary physical agony; it saves her also from some of the dangers attendant upon parturition, by husbanding her strength and warding off the effects of that exhaustion and nervous depression which the pains and shock of delivery tend to produce. In most cases the mothers, after delivery, on waking from their anæsthetic sleep, have expressed surprise at their own feelings of strength and perfect well-being; and many, who have borne children previously, have gratefully declared to me the great difference which they have found between their condition after being delivered under anæsthetics, and without pain and suffering, and their state of prostration after former labours, when they were subjected to the endurance of all the usual “pangs and agonies” of parturition. Nor does the benefit end here. By annulling the parturient pains and shock, and their direct and primary depressing effects upon the constitu-

tion, we ward off, I believe, to a more or less marked extent, the chances and dangers of those secondary vascular excitements which are always apt to follow indirectly upon them. We increase the chances of a more speedy and a more healthy convalescence. And both patients and practitioners have, as a general rule, had occasion to observe, that the period of convalescence has been evidently curtailed and shortened by the previous adoption of anæsthesia during delivery.

Such certainly has been my own experience. For, since following the practice of anæsthesia, my strong conviction is, that I have seen both more rapid recoveries than formerly, and fewer puerperal complications. One patient, however, had a short attack of peritonitis, requiring leeches, &c. It was her third accouchement and her first living child; and, after her two former deliveries, she had required to be bled, and treated for similar inflammatory attacks. At her first labour she suffered severely from puerperal convulsions. In two others of my patients the convalescence was delayed, in one by an attack of the affection described by Dr. Marshall Hall as "intestinal irritation" in puerperal females; and in the other by a fit of jaundice, which supervened two or three weeks subsequently to delivery, and after the patient had been for several days in the drawing-room. In December and January last, an epidemic of puerperal fever swept fatally over Edinburgh and other parts of Scotland. During the period of its prevalence, two of my patients were seized with it and died. But the previous employment of anæsthesia in these cases had nothing to do with this distressing result. Some of my professional brethren here and elsewhere, who were not using ether or chloroform, were much more unfortunate than I was. In a district in the neighbourhood of Edinburgh, one of the medical attendants informed me that, at that time, above twenty mothers were attacked and died, and in none of them whom the disease seized upon, did ether or chloroform happen to be used; while several who demanded chloroform during their labours, all fortunately escaped. The first of the two cases which I met with, was after a second labour. The patient's first labour was extremely tedious and prolonged, and, at last, symptoms supervened which demanded the delivery of the child by the forceps. In her second delivery, the labour was much shorter; the second stage lasted only for about twenty minutes, and during it she was completely anæsthetized. For fifty hours after delivery she progressed most favourably; and, after seeing her at that time with a pulse at eighty, and otherwise well, I was suddenly summoned, in consequence of extremely severe pain having come on in the uterine region after some muscular exertion. Rigors, rapidity

of the pulse, tympanitis, &c., supervened, and she speedily sank with all the usual symptoms of puerperal peritonitis. The second case alluded to was in a primipara. The labour was tedious, the pain severe, and the patient was anæsthetized for four or five hours before delivery. For some days after delivery she went on prosperously, until she became unhappily and greatly excited by discovering intemperate habits on the part of the monthly nurse who was taking charge of her infant and herself. A fit of convulsions (a disease to which, in earlier life, she had been long subject,) immediately supervened, and recurred several times. Fatal febrile symptoms then set in, with tympanitis and excessive diarrhœa.

I may add that, in the period during which these 150 cases occurred, I have had under my professional charge 20 or 30 other cases of labour in which anæsthesia was not employed, from the rapidity and facility of the delivery, from the patient being too late to send for assistance, from an aversion on the part of patients to the use of anæsthetics, more especially when ether first began to be used during last year, or from other causes.* One of the children in these cases was stillborn, and a second died two or three days after delivery. Two of the mothers suffered from crural phlebitis; a third had a severe attack of puerperal fever, but recovered. Two others died; one of them under an attack of puerperal convulsions and coma, which supervened fourteen days after delivery (see details of it in *Monthly Journal* for 1847, p. 213). In the other fatal case, the patient, who had suffered much in her previous labours, came to the immediate neighbourhood of Edinburgh to be confined, and with the view of using chloroform. But the labour proved unusually rapid, and she was delivered before the call for assistance reached my house. Her recovery went on uninterruptedly for two weeks, when a severe attack of dyspnœa supervened. My friend, Professor Miller, her ordinary medical adviser, saw her in my absence, and suspected some acute affection of the heart. When we visited her together shortly afterwards, the symptoms were then apparently those of acute endocarditis. She was submitted to the usual antiphlogistic treatment, and in four or five days felt again so well as to insist with us upon being allowed to rise, which was forbidden. In the course of a few hours afterwards, another fit of dyspnœa suddenly supervened, and, before Mr. Miller reached the patient's house, she was dead. We did not procure an autopsy. If unfortunately she had used chloroform during the labour, as was her intention and wish, many of the objectors to its em-

* Since November last I have used chloroform in all the cases of labour, where I have been called in time, except two.

ployment would have, I fear, unhesitatingly attributed the fatal issue in this case to its previous employment.*

In addition to the 150 cases of artificial anæsthesia that have occurred in my own midwifery practice, and to which the preceding remarks apply, I have witnessed, during the last eighteen months, a considerable number of instances in which anæsthetic agents were employed in consultation and hospital practice; and I have frequently had recourse to their assistance in various obstetric operations that I have been called upon to perform, as in the separation and removal of the placenta, in various cases of turning, in one craniotomy case, and in several patients who required to be delivered instrumentally by the long or short forceps.† In all these varieties of opera-

* In order to show the caution that is necessary in reasoning upon cases of death, apparently from the exhibition of chloroform during surgical operations, I may add that, since November last, scarcely an operation has been performed in Edinburgh, without previous anæsthesia, except where the throat or mouth was the seat of incisions, or the operation itself slight and trivial. Amid all the numerous patients thus operated on in public or private practice, when under the use of chloroform, no kind of misadventure or accident has happened; except one case of temporary fainting, a few minutes after recovery from the state of anæsthesia, be regarded as such. On the other hand, among the *few* exceptional cases in which, since November last, patients have been operated on in this city without chloroform, two have died on the table. One of the two was being operated on by Professor Miller for a hernia, which had been strangulated for about fifteen hours; when, after the skin merely was divided, the patient complained of great faintness, vomited, and died with the operation unfinished. This occurred on the 8th of November, two or three days after the anæsthetic effects of chloroform were discovered, and nearly proved the first operation in which it was tried. The second case, a patient of Dr. Pattison, had an abscess high up in the neck, requiring simple puncture for its evacuation. He died without hemorrhage, or admission of air, or other apparent cause, a minute or two after the puncture was made. If chloroform had been used in these cases, would it not by some have been blamed for the result?

Twelve or fourteen months ago, Professor Syme was performing primary amputation of the thigh in the hospital, upon a patient upon whom there was no sign of reaction, and who was not etherized for the operation. "Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of immediate dissolution. Before tying (says he) the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr. Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life; but by and by the actions of the system were gradually restored, and maintained through the use of stimulants."—See *Monthly Journal of Med. Sci.*, vol. 1847-48, p. 76. Such dangerous symptoms, coming on in an anæsthetized patient, might have been mistaken for the effects of the anæsthesia.

Some time ago, before either ether or chloroform was used in surgery, Dr. John Argyll Robertson was called, a few miles out of Edinburgh, to perform the operation for strangulated hernia. After having shaved the groin for this purpose, his patient complained of sickness and faintness, and died before any incisions were made.

Last year, Dr. Girdwood of Falkirk came to Edinburgh to see the practice of etherization, in order to be able to apply it in a case of amputation. The day for the operation and etherization was fixed; but, some hours previously, sudden apoplexy came on, and the patient died.

† In one case of placenta prævia to which I was called, the mother had lost

tive practice, the previous superinduction of anæsthesia has appeared to me to be of the greatest and most undoubted benefit. For, besides freeing the mother from the additional corporeal suffering and additional mental anxiety attendant upon operative delivery, the state of anæsthesia enables the practitioner to apply any operative interference that may be necessary with more ease and facility to himself, and consequently also with more safety and success to his patient. When the state of anæsthesia is rendered adequately deep, it renders the patient quiet and unresisting during the required operative procedures; it prevents, on her part, those sudden shrinkings and changes of position which the boldest and firmest woman cannot sometimes abstain from when her mind and body have been worn out, as happens in most operative cases, by a previous long and protracted endurance of exhausting but still ineffectual labour pains;—the introduction of the hand into the maternal passages, or of the hand to guide our instruments, is greatly facilitated both by the passiveness and apathetic state of the mother, and by that relaxation of the passages which deep anæsthesia almost always induces; and, lastly, this state of relaxation and dilatability renders the process of the artificial extraction of the infant through these passages alike more easy for the practitioner, less dangerous for the child, and more safe for the structures of the mother. Besides, in midwifery as in surgery, the utility of anæsthesia before operating, is not, I believe, limited to the mere annulment and abrogation of conscious pain on the part of the patient, and the rendering of the operation itself more easy to the practitioner, but it adds to the safety of our instrumental or artificial interference. For, in modifying and obliterating the condition of conscious pain, the “nervous shock” otherwise liable to be produced by such pain, particularly wherever it is extreme in degree or duration, or intensely waited for and endured, is saved to the already tried and shattered constitution of the mother; and thus, an escape is so far gained from those states of immediate vascular and nervous depression, and of subsequent febrile and inflammatory reactions, that are always apt to follow more directly or indirectly in its train.

much blood, and her lips were pale, and her pulse very weak. On administering chloroform the circulation and pulse rallied,—I separated the placenta—no bleeding recurred; and several hours afterwards the child was born. The mother made an excellent recovery.

CHAPTER VII.

MODE OF EXHIBITING CHLOROFORM; DOSE, ETC.

IN the course of the preceding observations I have omitted making any remarks on the degree of artificial anæsthesia required in obstetric practice, with the exception of stating that when instrumental or operative interference is adopted, the anæsthetic state must be made adequately deep,—so deep, that the patient must be rendered quite passive and apathetic. In fact, when induced for operative purposes in midwifery, the anæsthetic state should be as complete and profound as when it is induced for operative purposes in surgery. But, in common cases of parturition, the anæsthetic agent employed, whether chloroform or ether, does not, in general, require to be given in such large doses as in surgical practice. And in obstetric practice, the rules which I have usually followed in exhibiting the chloroform (the only agent I believe now used in Edinburgh and most other places), are those which I briefly stated when first writing on the subject for the *Monthly Journal* in November last. “After the first full dose, a few inhalations, before or with each returning uterine contraction, are generally sufficient. The state of anæsthesia should be made more deep as the head is passing the perineum and vulva.”* I have elsewhere in the same *Journal* stated these rules at somewhat greater length.—(See No. for April, p. 762.)

Occasionally I have at first, and especially in the early stages of labour, given the chloroform in small doses only, so as to obtund or obliterate the sensations of pain, without altogether abrogating the state of consciousness. In many patients, this degree of anæsthesia, with the results stated, viz., the loss, in a great measure, of pain without the entire loss of consciousness, can be readily enough induced, and answers excellently well; but, as a general rule, it has appeared to me in some cases objectionable. For not unfrequently, small doses, such as produce this condition, are accompanied with excitement and talking; and sometimes patients have complained to me of this renewal of the chloroform in small doses with each pain, being accompanied each time with a renewal of the ring-

* *Monthly Journal*, vol. for 1847-48, p. 417.

ing in the ears, flashes of light, and other disagreeable sensations accompanying, in some persons, the primary effects of the inhalation. Besides, we are never thus sure that we are really saving the patient to the full extent by the means we are using. If, on the other hand, she happen to be thrown at once into a deeper state of anæsthesia, the chances of such inconveniences and drawbacks are avoided. Often, when the anæsthetic state is thus made deep from the first, the uterine contractions are arrested for a few minutes, but speedily return. In order to effect this, we take care that as soon as the patient is asleep—(and, in natural labour, we seldom or never require to push the inhalation so far as to affect the respiration, and produce noisy inspiration, and snoring as in surgery),—the chloroform should be withdrawn, and not reapplied again till the movements of the patient, or the state of the uterus, as felt through the abdominal walls, indicate a returning uterine contraction. A few inhalations given then, and repeated with each returning uterine contraction, keeps the patient in a state of unconsciousness; and this condition may be easily maintained for hours, by administering in this way the chloroform vapour with each pain, and withdrawing it entirely during each interval. The practice is not to be expected to come upon medical men by intuition; for, like all other practices, some care and experience is necessary in order fully to acquire and apply it. And the *two* main difficulties which every beginner meets with are these: namely, to keep the patient in a state unconscious of pain, and yet not so deeply anæsthetized as to have the uterine action interrupted. For too deep a state of anæsthesia in general interferes with the force and frequency of the uterine contractions; while a lesser degree of the anæsthetic state leaves these contractions unaffected; and a still smaller dose often excites and increases them,—the effects, in this respect, of chloroform upon the uterus, being similar to the effects of opium in different doses. But the influence of the inhaled agent passes off in a few minutes, differing in this respect from the more permanent influence of a drug when swallowed; and if, at any time, the anæsthetic effect is too deep, and the uterine action is in consequence impeded, all that is necessary is to abstain entirely from exhibiting the chloroform for a short time, till the parturient contractions have been allowed to come back to their proper degree of strength and frequency; and then the anæsthetic agency is to be sustained as before, by giving the vapour with every recurring pain, but in smaller doses, or for a shorter time during each pain, than was previously practised.

Anæsthetic vapours, when given in large doses, have less power of reining up the action of the uterus in the last than

in the first part of labour. And as the sensations of pain become more agonizing as the head is distending the perineum, and passing through the vulva, the anæsthetic state usually requires to be then rendered more deep and complete than in the early stages of the process; and in most patients this may be done without at all impeding the rapidity of the delivery. Indeed, in many women, this latter part of the process of parturition seems to be accelerated by the superinduction of anæsthesia; for the degree of relaxation of the muscular structures of the perineum and vaginal orifice, commonly resulting from it, usually more than compensates for any diminution of uterine action, that may occur. If in any instance it proves otherwise, and the depth of the anæsthetic state interferes too much with the parturient contractions, the simple remedy is that I have already mentioned—a diminution in the state of anæsthesia, so as to allow of a return and increase of the expulsive efforts of the uterus.

The degree and depth of anæsthesia which different patients are capable of bearing without the irritability and contractions of the uterus being impeded, appears to differ greatly in different persons. In some, a very deep state will still leave the uterus almost or altogether unaffected; in others, its action is interrupted by a comparatively slight degree of the anæsthetic state. It is this variability which at first forms the principal difficulty to those commencing the use of chloroform in obstetric practice. But experience and care will soon enable any attentive observer to overcome this apparent obstacle, and to adapt the dose of the agent to the powers and capabilities of each different patient. I have never yet seen an instance, but I can conceive it possible, that in some rare exceptional cases and idiosyncrasies, the action of the ether or chloroform should, even in such small doses as merely produce unconsciousness to pain, interfere, especially in the first stage of labour, too much with the muscular action of the uterus, and require to be given up, at least till the labour be more advanced. But this would, of course, be no reason for not employing it in those other persons in whom it had not such an influence; any more than because opium occasionally does not act as an hypnotic on particular patients, it should not be given with that indication to any other patients with the view of inducing sleep.

During the anæsthetic sleep which chloroform induces in natural labour, the patient usually lies perfectly quiet and passive in the intervals between the pains, but moves more or less, and sometimes moans, as each uterine contraction begins to return. In the last stage she generally, with every recurring

uterine contraction, makes the usual violent bearing-down muscular efforts, and the struggle can often be marked in the expressions of her face. The muscular action of the uterus and assistant muscles goes on, and yet she remains quite unconscious. The strictest quietude should always be observed and enforced around the patient, for noises and speaking, particularly soon after the chloroform is commenced, will sometimes excite and make her talk; and, if this happen, we may require to exhibit to her a deeper dose than would otherwise be at all necessary. One or two practitioners of midwifery in London have averred and repeated, over and over again, in our medical journals, and in pamphlets intended for non-medical readers, that obstetric patients, under the influence of chloroform, must be liable to talk and act grossly and obscenely. This objection to the practice of anæsthesia in midwifery has been repeated and gloated over by those who have propounded it, in a way forming, apparently unconsciously on their own part, the severest self-inflicted censure upon the sensuality of their own thoughts. An impure mind, more especially in a professional man, may easily fancy and find impurities where none whatever exist; but he is not on that account entitled to imagine, that his own lewd thoughts are typified in the thoughts or actions of his patients. In answer to the supposed objection itself, I have merely to observe, that I never once witnessed any trace of indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren, of whom I have inquired on the subject here, is to the same effect. In a paper on temporary delirium occurring in the course of labour, Dr. Montgomery several years ago described more marked instances of effects of this description, arising merely from "the extreme distress and pain" to which the mother was subjected in the dilatation of the os uteri, &c., during natural parturition, than were ever seen to arise from the influence of means used to abate and abrogate that "extreme distress and pain."

In administering chloroform in obstetric practice, I have always used the handkerchief, as the simplest and best apparatus. Sometimes, when the case is likely to be tedious, I have it folded and sewed into the form of a deep cup or cone. The chloroform is poured into the bottom of the cup, the open end of it held over the nose and mouth of the patient when the action of the vapour is required; and, when its application is suspended, by closing the open end of the cone, the escape and loss of the vapour is prevented during the intervals. Such an arrangement saves the chloroform. But a handkerchief merely folded together and sprinkled with chloroform, answers quite

well; and in the intervals it may be compressed together in the hand, so as to prevent the escape of the chloroform. In *first* throwing the patient over into the anæsthetic sleep,—(the point which requires the most management),—a handkerchief thus presenting a large surface is often much more serviceable than one folded into a cup shape; for the patient, when first coming under the influence of the chloroform, is apt to move her head from side to side; and, in order to keep up the constant inspiration of the vapour, she can be more easily followed by using a simple handkerchief, than by trying to keep any kind of apparatus applied to her mouth or face.

The quantity of chloroform used varies both according to the duration of the labour, and the susceptibility of the patient. Usually, when the handkerchief is used, about an ounce an hour is necessary, a small quantity being poured upon it from time to time. A less dose will suffice in some, and others require more. In one case lately, where the patient, in a first labour, was anæsthetized for two hours, I expended nearly six ounces, large doses being necessary to keep her in a sufficiently deep state of unconsciousness. The first quantity which I pour on, usually amounts to three or four drachms; but I always judge by the *effects*, not by measuring the dose; and I pour on an additional quantity in a minute or so, if it be required. In holding the handkerchief towards the patient, I take care that plenty of atmospheric air is admitted,—and seldom or never put it in contact with the face. At first, it is better to hold it at a considerable distance, in order to prevent any chance of irritation and coughing; and then gradually approach it. It is always to be remembered, that the vapour of chloroform is nearly four times the specific gravity of atmospheric air; and if the patient is lying on her side, the handkerchief or pillow can be easily arranged so as to keep a larger supply of this heavy vapour opposite the mouth and nostrils. I have always held and managed the handkerchief myself in the first instance, and till the patient was asleep. Afterwards, I have generally trusted it to the husband or nurse, teaching them to apply it near the face when the pains supervened; and folding up the handkerchief in the way mentioned, so as to preserve the chloroform during the intervals.

When exhibiting chloroform in obstetric practice, and in the way I have described, I have often been struck by the circumstance, that its use is very rarely followed by sickness or vomiting. I do not remember having seen vomiting follow its exhibition in labour in more than four or five cases, and two of these (in the practice of Dr. Paterson and Dr. Cochrane), were instances in which I was called in to apply the forceps, and where the

patients were placed for the operation in a state as deep as that used in surgery. I have repeatedly seen it arrest the sickness and vomiting occasionally accompanying the first stage of labour.

In addition, let me state, that I have usually begun the employment of the chloroform when the os uteri was well dilated, or towards the termination of the first and the commencement of the second stage of the labour. But when the pains were severe I have commenced it earlier, and when the os uteri was still comparatively little dilated. There is, I believe, no limit as to the date of the labour at which we may give it.

CHAPTER VIII.

REPORT OF THE RESULTS OF ANÆSTHESIA FROM DIFFERENT OBSTETRIC HOSPITALS AND PRACTITIONERS.

THE following account of the results of anæsthesia in the practice of the Maternity Hospital, Edinburgh, has been drawn up by Dr. Duncan and Mr. Norris, two gentlemen who have acted as resident house-surgeons in the institution, and upon whose power and accuracy of observation all who are acquainted with them will place implicit reliance.

"Since the use of anæsthesia in labour became general in the Maternity Hospital, shortly after the discovery of chloroform, 95 women in all have been delivered in the house under its influence. Among these, 88 were natural and 7 were morbid labours. In the 88 cases of natural delivery, only one of the mothers died, convulsions coming on five hours after delivery, and proving fatal after a continuance of six days. On a *post mortem* examination, the kidneys were found to have undergone, in some parts, the true steatoid degeneration.—(See a report of the case, *Monthly Journal for September*, p. 196.) Among the same 88 cases of natural labour, there were 5 dead-born children. In 2 of these cases, the birth was premature, being at the sixth month. In the third case, the mother had previously given birth to two dead infants. The fourth dead child had a very large hydrocephalic head. The proportion of still-born children was thus 1 in 17. In the Dublin Hospital the proportion of still-born children, as reported some years ago by Dr. Collins, was 1 in 15. The 7 instrumental cases were as follows;—1 application of the short forceps, in an unsuccessful attempt to save the life of the child; the mother recovered well;—2 cases in which the long forceps were applied; one of the women, in whom the head was very long impacted in the pelvic brim, died from sloughing of the maternal passages; the other made a good recovery; both the children were born alive;—4 cases of version; one of the mothers died from rupture of the uterus, the others recovered quickly; 3 of the children were still-born, and in one of these 3 cases the cord was prolapsed.

"In addition to these eighty-eight cases of anæsthetic delivery, there have been upwards of fifty women delivered in the house without chloroform. These have been chiefly very rapid labours, where the women have come into the hospital just in time to give birth to their infants, or where the house-surgeon has not been able to see them till very shortly before delivery. From the expense attending a large consumption of chloroform, it has always been an object to husband it as much as possible;* and therefore, in the hospital, it has not been given in cases where the mothers did not very severely complain of their sufferings, nor were harassed with feelings of anxiety and fear.

"On the whole, the results of anæsthetic midwifery, as observed by us in the Hospital, have been perfectly satisfactory; and we can confidently state that the recoveries have been altogether more perfect and speedy than before. This has been remarked in so great a proportion of the cases, that there can be no doubt whatever of the truth of the observation. Besides the increased rapidity of recovery, we have noticed the almost entire absence of those uncomfortable feelings of fatigue, languor, and shivering, and of that shattered feeling which so frequently comes upon the mother immediately after an ordinary delivery. Instead of this, we have found the mother almost invariably awake from the anæsthetic sleep comparatively fresh, easy, and cheerful. Not unfrequently the anæsthetic has been found to change, without an intermission, into a natural sleep, which may continue for an hour or two.

"Further, there have been, since the introduction of chloroform into the practice of the hospital, far fewer than formerly of those violent attacks of rigors, ephemeral fevers or weeds, and abdominal pains, which are so common in most crowded hospitals, forming a class of cases which used formerly to cause much anxiety, and was a common cause of the mother's being detained in the hospital after the usual fortnight allowed for recovery. In fact, since using chloroform, there have been scarcely any women detained in the house by these causes, and much less Dover's powder, calomel and opium, abdominal fomentations, &c., have been used.

"The women have been, invariably, found deeply grateful for the relief to their sufferings afforded by the anæsthetic influence of chloroform.

"Yours, &c.,

"J. M. DUNCAN. H. NORRIS."

* Perhaps, in a short time, a benevolent government will allow chloroform to be made cheaper, by removing the very high duty on proof spirit when used for medical and chemical purposes (tinctures, &c.) At present that high duty is, in one respect, a direct tax upon the relief of human disease and the mitigation of human pain; and a great obstacle to the progress of British organic chemistry.

At a meeting of the Edinburgh Medico-Chirurgical Society in June last, along with other practitioners, I gave in a report on the employment of chloroform in midwifery. At that time, and since, I have been favoured with written statements of the results by various medical friends in Edinburgh, and in different parts of the country. I shall now give extracts from a variety of the letters which I have received relative to this subject. Many more such communications might easily have been called up and adduced; but I have deemed it useless to multiply unnecessarily this kind of evidence. It will be observed, that, with one exception (see the communication of Mr. Lansdowne), the following letters refer—like the preceding statements regarding the Maternity Hospital—to the use of chloroform alone.

The first statement which I give is from my assistant and friend, Dr. Keith.

“I have employed chloroform in every case of labour under my care, since its introduction, with one exception; and also in almost every case to which I have been called in by other practitioners. In my own cases, amounting to about four-and-twenty, it has been given for a period varying from half an hour to eight hours. The quantity of chloroform consumed has been, on an average, about one ounce per hour; in a few cases double this quantity was found requisite. The anæsthesia has been in almost every case complete; that is, the patient, on awakening, has declared, that while under the influence of the chloroform, she was utterly unconscious of all pain. In most cases the patient has lain quiet even during the pains, the presence of which is then generally indicated by the breathing becoming more rapid and somewhat laboured. In other cases there is suppressed moaning during the pains, or even, in some, loud manifestations of powerful straining and muscular exertion. I can state most positively that I have seen no serious symptom which could be traced to the chloroform, in any one case, either as affecting the mother or the child. Most of the mothers have made uncommonly good recoveries. Those who have had children previously, have, almost without exception, stated to me, that they felt very decidedly stronger after delivery than on former occasions. In two cases the recovery was rather slow, but this was owing to the patients' having been in a very delicate state during pregnancy,—and, in both instances, I considered the chloroform was of very great service, by saving their strength. All the mothers are now in their usual health.

“In no one of the twenty-four cases was the child still-born. In one case labour was brought on at the end of the seventh month, owing to the brim of the pelvis being much contracted.

The child was born alive, but died on the second day. All the other children are now alive. They have all been nursed by their own mothers, with one exception.

"I have had occasion to use the forceps seven times since the introduction of chloroform, and once to break up the child's head and extract by the crotchet. In all these cases the patient was first put into a deep anæsthetic state, and in most she lay perfectly still and apathetic during the operation. All the mothers have done well, except in the case of craniotomy, where the uterus had ruptured previous to the use of chloroform.

Yours, &c.

"G. S. KEITH."

From Dr. Moir, Edinburgh.

"Since the beginning of December I have, with a very few exceptions, used chloroform in the course of my midwifery practice, and I have not met with a single case where any unpleasant effects, either to mother or child, can be traced to its use.

"As far as my observation has gone, I think it will be found that, in some cases, the chloroform, if freely administered at an early period of the first stage, retards the pains a little, and in others also lessens their power; and when this does occur, the best remedy is either to intermit its use till the labour is further advanced, or to give it in smaller quantities and at longer intervals, so as not, at that stage, to induce complete unconsciousness. But, whether correct or not in this opinion, I am quite satisfied that the second stage is much accelerated, especially towards its termination, by the chloroform doing away with the resistance offered to the expulsion of the head by the muscles at the outlet of the pelvis,—and this to such an extent, that, in some *first* cases, there is a risk, unless very great care is taken, that the perineum be slightly lacerated, from the head being so rapidly expelled as not to give time to the parts to yield so rapidly as they would otherwise do. But this is comparatively a very rare occurrence, and requires to be mentioned principally with the view of putting young practitioners on their guard against it, and of leading them to use the necessary means to prevent it.

"In exhibiting so powerful an agent as chloroform, I think it a point of importance to use as small a quantity as is compatible with the obtaining of its full anæsthetic effects; and, as this seems to depend much upon the rapidity with which it is conveyed into the system, it seems a point worthy of consideration to ascertain the readiest means of so doing. Various instruments have been invented for this purpose, though they have

been almost universally superseded by the use of the handkerchief, as recommended by you, and used either in the form of a hollow cone, applied again and again, after renewing the chloroform, over the nose and mouth of the patient; or simply folded up several times, and frequently having interposed between the folds a piece of wool or flannel, with the view of better retaining the chloroform. In both methods there is a considerable loss of chloroform, much of it being retained in the folds of the handkerchief, and much of it escaping without passing into the lungs, or else passing into them so slowly as not to produce the desired effect. I have, for some time, been in the habit of using a linen or white cotton handkerchief, folded only once, or, if very thin, folded twice; the point requiring attention being, that it should not be so thick as to offer any impediment whatever to free respiration when applied over the mouth and nostrils. Since using the chloroform in this way, I have never failed in rapidly producing the anæsthesia, either in my own practice, or when accompanying some of my patients (who were several months advanced in pregnancy) to their dentists to have one or more teeth extracted. For administering the vapour to patients who are in the erect position, the chloroform should be poured on that part of the handkerchief placed on the palm of the operator; the edge of the little finger should then be applied close to the chin, and the hand gradually raised up towards the mouth, till the sensation of choking which generally accompanies the first inspirations has passed off, after which, the handkerchief should be left on the face and the hand removed; the patient then breathes freely through that part of the handkerchief wetted with the chloroform, and, in general, half a drachm is sufficient to produce anæsthesia.

"In obstetric practice, I find it the most convenient plan to place one end of the folded handkerchief under the left cheek of the patient, to pour a little chloroform on it, and then, taking hold of the loose end of the handkerchief, to bring it gradually near the mouth, till it can be left there without inconvenience, the patient breathing freely through it. And it is not necessary again to remove the handkerchief, but simply to pour on it occasionally a very few drops, whenever the patient begins to show symptoms of returning consciousness, or on the accession of a pain. The only precautions necessary are, to raise a small fold of the handkerchief from the skin when the chloroform is to be applied, so as not to blister the skin; and to drop the chloroform, not on that part of the handkerchief immediately over the mouth, but a little above it, so that the vapour, being heavy, may flow down towards the mouth or nostrils,

and thus be, during inspiration, more readily received into the lungs. By adopting this plan, I have had the handkerchief applied for nearly two hours without removing it; and the quantity of chloroform I have used in single patients, has been much less than other practitioners have used in cases of the same duration.

“Yours, &c.

“JOHN MOIR.”

From Dr. Malcolm, Edinburgh.

“Since November last I have employed chloroform in above thirty cases of labour, and with the most satisfactory and delightful results. A majority of these were first labours. I have kept my patients under it for periods varying from half an hour to six hours, and have never found the slightest unpleasant effects result from its use. All the children have been born alive, and are at this moment in perfect health, with the exception of one that died when about a month old, of a sudden and severe attack of dysentery. All the mothers have made recoveries with rapidity and completeness, far above the average which I had previously observed in my practice. This has struck me as the more remarkable, seeing a large proportion of my patients were primiparous; and I can only attribute this result to the entire absence of suffering and shock to the nervous system which is effected by the use of chloroform. Although in a few cases my patients and their friends have at first objected to the use of anæsthesia to abolish pains which they considered “natural,” yet every one has afterwards expressed to me sincere gratitude for saving them from their agonies; and I am sure not one who has experienced the beneficial effects of the practice will ever submit to these agonies again, now that they know that they are so totally unnecessary, and can be so easily and safely abolished. I have repeatedly found the mothers of my patients object to anæsthesia, as if they grudged that their daughters should not experience the same sufferings as themselves,—but I have uniformly found them afterwards as grateful as their daughters for the relief administered.

“Generally, I have employed about an ounce of chloroform per hour. I have never seen the uterine contractions arrested by its use, although I have no doubt a large dose would, when necessary, have that effect. I have seen no case of hemorrhage, or convulsions, or any other complication whatever. Let me add, that I cannot conceive on what principle the employment of chloroform in natural labour should be objected to,

as long as it is our duty, and assuredly it is our duty, as physicians, to relieve and mitigate human suffering.

“Yours, &c.

“R. B. MALCOLM.”

From Dr. Thomson, Edinburgh.

After stating the details of ten cases, Dr. T. remarks:—

“Among the middling classes I have met with more difficulty in using the chloroform than I had anticipated, as it has only been when the sufferings of the patient were very severe, or her friends had begun to dread the effects of prolonged continuous suffering on her constitution, that I could get my wishes carried into effect. That fatal Newcastle case, which was trumpeted a good deal in the newspapers, is still haunting their minds, and is very frequently urged by some timid friend when you propose the chloroform to relieve the sufferings of the patient.

“My experience of it has been, in all obstetric instances, analogous to your own; with one exception, I have had no difficulty in getting the patient under its influence: a minute or two was in general sufficient to lay the most restless or ungovernable patient quiet on her pillow.

“Not the slightest *post-partum* hemorrhage has taken place in my ten cases, though in the two forceps cases, where it was given deeply, the uterus remained flabby for nearly thirty or forty minutes, and threw off the placenta with difficulty.

“Its relaxing effects are, I think, undeniable. In one case, the soft parts had resisted for a considerable time the descent of the head; they yielded very readily within an hour after the chloroform was begun. I have not had another instance of this kind lately; but, were I to meet with one, I feel confident it would yield with much more facility under the chloroform than without it.

Yours, &c.

“ALEX. THOMSON.”

From Mr. Carmichael, Edinburgh.

“I have given the chloroform in twenty-six cases of midwifery, four of which were first labours; the others varied from the second to the eleventh pregnancy. The quantity given varied from two drachms to four oz., and the length of time during which it was exhibited from a few minutes to four hours. The preparation I have used has always been that of Messrs. Duncan, Flockhart, and Co., and I have never seen the slightest bad effects from it, either in midwifery or other

medical cases, or in any cases where I have administered it for amusement, except occasional sickness where it was exhibited shortly after a meal.

"I have met with no case of flooding whatever. I have heard it alleged that it drives away the milk; but I have not found it so, as my patients have all been able to nurse, with the exception of one lady, who has not been able to suckle her child for the last three times.

"In all of these cases it was administered with the greatest ease and with perfect success, and, in no case, with any bad results.

"The recoveries have been certainly more than usually speedy. Indeed the only objection I have met with as to its use, has been on the part of the monthly nurses, who seem afraid that the new practice will curtail their attendance and pay.

"All the children were born alive, and are doing well. The only case in which the child proved the least refractory was a footling one.

"I have also found it most useful in cases of dysmenorrhœa, in spasmodic colic, and tic douloureux.

"I also gave it, in a case which you saw along with me, of most severe neuralgia of the uterus, with the most perfect success.

"In no case whatever have I seen any bad results of any kind arise from the use of the chloroform.

"Yours, &c.

"W. S. CARMICHAEL."

From Dr. Burn, Edinburgh.

"I regret that I cannot give you the number of cases of labour in which I have exhibited the chloroform, but I may state that I have given it repeatedly, and have not seen any bad consequences either to the mother or child result from its use.

"All the mothers made rapid recoveries, and the children did not appear to suffer from its use.

"I have given the chloroform in three or four cases of adherent placenta where the uterus was firmly contracted, and had far less difficulty in extracting it than I have experienced in similar cases where the chloroform was not exhibited.

"Yours, &c.

J. BURN."

From Dr. Purdie, Edinburgh.

"I have now used chloroform in seventeen cases, which I have noted, and in every instance with decided effect, not

merely by lessening suffering, but I am perfectly convinced, by the most careful observation, by shortening the duration of labour. The pains have never in my experience been interfered with, except by rendering them quicker, and far more effectual.

"There is one of the cases which I would wish to recall to your memory. The patient, thirty-eight years of age, was in her first labour, which commenced early on Wednesday morning, and went on well but slowly till the evening, when its progress ceased, although the pains continued regular and strong. The os uteri was well dilated, but the head made no progress, although there was no very evident cause. About one o'clock on Thursday morning I sent for you to deliver her with forceps. On your arrival, you thought that still there was hope of the labour being terminated naturally. The patient, who was suffering much, was then put under the influence of chloroform and ergot, while you waited patiently for any advancement, for nearly two hours, without effect. You then delivered with the long forceps, which cost you great exertion, from the head being impacted in the brim. The patient's position was changed, the placenta was extracted, she was bound up and laid in a comfortable and easy posture, in which state she continued to sleep soundly until she was awakened after the child was dressed, the crying of which surprised her, as she had not been conscious of what had taken place from the time she got the first dose of the chloroform after your arrival. This patient had an excellent recovery. I never saw a patient suffer less after labour, or recover more rapidly. I may just add, that there are few things vex me more with regard to patients, than to witness the sufferings of a childbed patient, who will not allow, from ignorance or prejudice, the use of chloroform. Happily, however, such cases are very rare among us.

Yours, &c.

"W. PURDIE."

From Dr. Finlay, Newhaven, near Edinburgh.

"I have used chloroform in a considerable number of cases of natural labour. It was with much reluctance that I first administered it, and only at the urgent entreaty of a patient who was enduring intense agony before the birth of a first child. It was completely successful. Her screams had been audible across the street. In a few minutes they ceased, and she fell asleep, while the uterus continued to act as powerfully as before. She was not aware that she had got her baby until a quarter of an hour after it was born. In five of the other cases the influence of the chloroform was as complete. In the other

three cases *entire* insensibility was not induced, but the sufferings were greatly mitigated, and the relief was so evident to the patients, that, whenever the labour pain was approaching, they grasped with great eagerness the handkerchief on which the drug was sprinkled. Satisfactory as these cases have been, I have hitherto used the chloroform with considerable hesitation and caution, and only when it was asked for by the patient. But every trial has emboldened me to employ it with greater confidence on future occasions. In each of my cases the placenta was soon and easily detached; in none of them did hemorrhage occur; and they all made excellent recoveries.

"Yours, &c.

A. FINLAY."

From Dr. Cumming, Edinburgh.

"I have now attended thirty-five cases of labour under chloroform, and it has been used in all with marked advantage. All the patients have made unusually good recoveries; and I have been very much impressed by the fact, which was remarked by the first patient submitted by me to chloroform, and repeated by all, that the convalescence was not accompanied by the crushed and dislocated feeling that they have experienced without it.

"I always begin by introducing the chloroform slowly and gradually into the lungs, allowing a large proportion of air to be inhaled along with it. In every instance it was administered, not with reference to the quantity given, but to the effect produced,—this effect being complete unconsciousness during the pain; and thus administered, I have never seen any unpleasant or absurd consequences, nor anything to excite alarm or even uneasiness.

"Two of the cases had had large floodings in a previous labour; with the chloroform there was none. This, of course, I do not impute to the medicine; but it at least tends to prove, that hemorrhage is neither a necessary nor a likely consequence, as many at first were disposed to imagine.

"All the children were born alive, and are so still. None of them as yet give the slightest indications of idiotcy, either present or future; nor have I observed in any the temporary stupefaction immediately after birth, ascribed to the presence of chloroform in the apartment, that some have remarked.

"I am quite satisfied that, if properly given, it acts as a calmative; and I believe, from what has passed under my observation, that very many of what are called exceptional cases are not so in reality, but appear to be such from error in the mode of administration, and that further experience will amply demonstrate the truth of this.

"In short, I am, unfortunately for the appearance of veracity, compelled to say, that all my cases hitherto have been so successful, the recoveries so uniformly good, and the satisfaction on the part of the patient (I may add also my own) so great, that I am rapidly approaching to, if indeed I have not already arrived at, the conviction, that, *if there be any sin connected with chloroform, it is chargeable on those who refuse to administer it.*

"I may add, that not one of those patients who have already inhaled it will ever be denied it in any subsequent pregnancy, as they have repeatedly assured me; and certainly I shall not attempt to keep it from them, and that not more for their sake than my own.

"Yours, &c.

"W. CUMMING."

If necessary, I might have adduced more evidence in favour of the anæsthetic effects of chloroform in midwifery practice, from Dr. Beilby, Dr. Zeigler, Dr. Weir, Dr. Young, Dr. Menzies, Dr. Gilchrist, Dr. Campbell, and other medical practitioners in Edinburgh who have been using it.

In order to vary the kind of evidence, I shall next adduce extracts from various communications which I have received on the use of chloroform in midwifery, from medical correspondents in different parts of Scotland, England, and Ireland. It is needless, I believe, to attempt to arrange them in any special order; and I shall content myself, therefore, with beginning with the letters of those practitioners who live farthest north, and proceed southward.

The following is an extract from a letter written to me this summer:—

From Dr. Grigor of Naim.

"Dr. Allan of Forres and myself would as soon think of going to an obstetric case without our chloroform phial, as we would of going to bleed a patient without a lancet. In this quarter, doctors are only called in when things are going wrong, or in extreme cases; so that, since your grand discovery, he and I have only used it in about twenty-four cases, in all which it came up to all you have written about it,—no still-born children—mothers recovering well—fewer after-pains, &c. &c. One of my cases was a first child, the mother nearly forty-eight years of age, weakly in constitution, and of small formation. Had it not been for the chloroform, I do think she would have sunk.

"Yours, &c.

"J. GRIGOR."

Dr. Dyce, lecturer on midwifery in Marischal College, Aberdeen, favoured me some time ago with the following interesting communication, regarding the obstetric employment of chloroform:—

“I have reports from my friends Drs. Harvey, Pirrie, and Gilchrist, all of whom, I was aware, had been employing it. I may at once state that I consider it a most invaluable agent; that I have every confidence in its safety; that I recommend it almost on every occasion; that no evil consequences have ever attended its use; and that I have found its effects nearly alike in all. I have used it eleven times. The labours have, with one exception, been natural. The exception was a breech case, and a first child. Two of the eleven children were lost; one was putrid, and in the other (the breech case), though the funis did pulsate for some minutes, the child could not be recovered.

“Dr. Harvey has given me a brief account of four cases delivered under chloroform; all the children were born alive.

“Dr. Gilchrist at Woodside writes to me, ‘I have observed no evil results to the infants themselves from the use of chloroform.’ He does not state the number of his cases; but I presume they have been numerous from his remarking, that ‘latterly I have not used this agent so generally as when it was novel, reserving it now, unless when urgently requested by the patient, for cases unusually painful, whether arising from excessive sensibility of the system, rigidity of the soft structures, or cases requiring manual assistance.’

“Dr. Pirrie has employed chloroform in fourteen cases. Ten of these were natural, two instrumental, and one a case of turning. He says, ‘as to the children, they have all been born alive, and continue to go on satisfactorily.’

“I never use it early in labour; generally the second stage has come on, or at least the os uteri is tolerably well dilated. This appears to be the practice of my brethren here. I then keep the patient in a state of insensibility more or less complete during the future progress of the labour. Occasionally I have allowed them to come completely out of the anæsthetic state; but, on the recurrence of the pain, the patients invariably and urgently seek for the handkerchief. When I used chloroform, I imagined that the intenseness of the stupor induced, did lessen the frequency and force of the pains, and even put a stop to the labour altogether; and I am still of opinion, that if the full effect is produced and kept up for any time, the pains will cease; but if a more moderate effect is produced, so that consciousness to a certain extent remains, my conviction is, that instead of checking uterine action, its use enables the organ to act with more freedom; the mental influences are quickened; and, more than

this, it has a decided power in relaxing the soft parts, and thus removing a very frequent cause of protracted labour. I had a very interesting case illustrative of these remarks in January last, in a lady in her fifth pregnancy, of a very irritable, anxious, and highly nervous temperament, who had previously to labour determined to use this wonderful agent. Her former labours had always been tedious, and very painful throughout every stage. This commenced and had continued for a couple of hours before I saw her. She was complaining much of the severity of the pains, especially of her inability to move from one posture. Her anxiety and agitation were very considerable; her pulse was above 100, and her body was already wet with perspiration. On examination I found the parts rigid, and the os not larger than a shilling, while the rectum actually encroached upon the vagina from its loaded state. I determined, therefore, on emptying the gut by an enema before employing the chloroform, and mentioned this to my patient, and gave the nurse the necessary instructions. I very soon found that I had added not a little to her already excited state, as she was not prepared for delay, and would only, after some persuasion, consent to its exhibition, and only then on condition that immediately after she was to be allowed the 'stuff.' Her request was complied with, and I confess to you its effect on her general state from the moment she was under its influence, not only gratified myself, but pleased her friends, who complained much of her impatience and her imagined evils. A few inhalations sufficed to induce insensibility, before which she gave one or two hysterical laughs, and made some incoherent remark. She then became still and quiet, until a pain returned,—the only indication of which was a gentle writhing of the body. Her pulse very soon fell to 80, at which it remained; her skin became cool; the parts rapidly relaxed; she moved, and allowed herself to be moved in any direction readily: the pains were certainly more regular and efficient than in any of her former labours; and in two hours and a half from the first inhalation, the child was born. Once only during this period, and that was after one hour had elapsed, was she perfectly sensible. This was permitted at the solicitation of the nurse and friends, who had not seen chloroform used before, and could not be convinced that so sudden a change could exist and the labour progress perfectly. They soon had evidence of this, by the patient screaming out in her former impatient tone, 'Pain, pain! where is the handkerchief?' From this time until delivery she was perfectly unconscious. The placenta came away naturally in ten minutes. The roller was put on, some of the soiled things were changed, and it was not for several minutes

ere she was convinced that her trials were over. I need hardly say that she was gratified in the highest degree to find that she had escaped so much of her former sufferings, and expressed her deep thankfulness for so valuable a boon. The child was putrid. Her recovery was perfect, and more rapid than on any former occasion.

"In my practice I have met with no instance of chloroform failing in producing its usual effects in a very few minutes, where it was willingly and readily inhaled; in some nervous patients, a few seconds are sufficient for this purpose. In the case I have just given, I was surprised at the few inhalations she took; and Dr. Pirrie mentions that one of his patients was so highly susceptible of its effects, that, on making three or four inhalations, the state of anæsthesia became so complete, that the labour was entirely suspended; he therefore, in this case, discontinued its use. On some occasions I had difficulty in persuading the patients to use it; hence a longer time elapsed. In one case when persuasion failed, I watched an interval of drowsiness, and in spite of herself brought the woman fairly under its influence, in which state she remained for twenty minutes, when the child was born unconsciously. Its cry aroused her; she turned herself hastily around with an inquiring look, and after a momentary gaze said, 'What is that? you have been giving me the stuff.'

"The quantity of liquid used has varied in my hands from 3ss to 3iiss. This last quantity was expended in the case of a young woman, of eighteen years of age, pregnant of her first child, and who was more or less under the influence of chloroform for five hours. As this was amongst my first cases, if not the very first, and the longest period in which I have employed this agent, perhaps you will excuse my giving it somewhat in detail, more especially as ergot was given along with it, which I am not aware that I had seen anywhere recommended. Jane ———, æt. eighteen, unmarried; first child, December 18, 1847. When seen at five P. M., the membranes were reported to have ruptured twelve or thirteen hours before; on examination, the os uteri was found dilated to the size of half-a-crown, and the breech was discovered as the presenting part; the pains were frequent, irregular, at times only effectual, but they had been increasingly severe during the after part of the day, and now were strong.

"About seven P. M., after some persuasion, she first inhaled the chloroform. She soon became insensible; it was then only presented to her at intervals as a pain recurred, a single inhalation being generally sufficient. She laughed and sang for a few minutes, but at length seemed perfectly unconscious, and

we were only sensible that she had pain by her now and then rolling about, as is the case when labour-pains continue under convulsions. For an hour she was kept in this state; but thinking the pains were becoming more distant, and the progress of the labour slower, she was allowed to come out of this torpid state. Soon after 3ss. of powdered ergot was given, and repeated four times at intervals of a quarter of an hour. The pains now became stronger and more frequent, and my patient, who was conscious of the powerful agency of the chloroform, and the advantages she had derived, insisted on its repetition. She had, in fact, secreted the handkerchief, and was now keeping it at her mouth. But, alas! its talismanic powers had vanished; she threw it rather angrily at the student, who, at that moment, sat by her bed-side, desiring him to give her the bottle in its stead; she assured us that she had felt no pain until now, that she had been dreaming of her illness, which she believed was finished. From this time (nine o'clock) until her delivery, which took place between one and two o'clock the following morning, she was kept in a perfectly unconscious state, at times more so than at others, according to the severity of the pains. Towards the end of the labour the throes were very severe, but during none of them did the patient complain of the slightest pain. The only indication, as I have already said, of apparent suffering, was a movement of the body, and occasionally a low moan. The child in this case did not breathe or cry; it was not therefore legally in life, though the funis pulsated for some minutes.

"I can scarcely think that the death ought to be attributed to the chloroform, or even to the ergot. The length of the labour, the nature of the presentation, and especially its being a first child, were almost against its being born alive. So that, with this exception, no injury has befallen the infant in the practice of any of us here who have employed chloroform.

"I have only further to say, that the recoveries in every case have been most satisfactory. They appeared even more rapid than under ordinary circumstances.

"Yours, &c.

"R. DYCE."

From Mr. Lawrence, Montrose.

"I am sorry I cannot furnish much information as to the employment of chloroform in midwifery in this quarter. It has been very little used by myself or brethren, owing chiefly to a very general prejudice on the part of our parturient patients against it. The very last case I attended, the patient resolutely refused it, although suffering very severe pain! However, in

one of those in which I administered it, and to which I had gone unprovided with the chloroform, my patient compelled me to send for it! In one case in which a patient of mine had convulsions in her first labour, I exhibited chloroform at her second confinement with the usual success.

"I have seen no injurious consequences of any kind.

"Yours, &c.

SAMUEL LAWRENCE."

From Dr. Steele, Montrose.

"I have used chloroform in midwifery practice on six separate occasions. In two of the cases, it was given to the extent of producing only a partial immunity from suffering; but with the effect of converting, especially in one to whom it was administered for four hours, what would have been a very severe, into a very easy labour. In the other four, the effect was highly satisfactory; two of the children being born without the consciousness on the part of the mothers, for some time after, of that event having taken place. One lady remarked, that she thought 'the march of intellect had never taken a happier direction than when it led to the discovery of chloroform.'

"All the children did well. One of them, however, was asphyxiated for nearly a quarter of an hour after its birth.

"From the little experience I have had of chloroform, any opinion of mine as to its merits is not entitled to much weight. It seemed certainly in several of my cases to prolong the interval between the pains, and thereby retard the termination of the labour; and I thought also that the uterine action was sometimes less effective when the patient was under the power of the medicine.

Yours, &c.

"GEORGE STEELE."

From Dr. Paton, Dundee.

"I have ascertained that chloroform has been employed in upwards of fifty cases up to this date (5th June, 1848), several of them instrumental, and with the best effect; and no bad consequence has attended it either to the mothers or children. All the children have been born alive. In the cases in my own practice in which I have used it, it speedily induced the anæsthetic state, and appeared to accelerate the uterine action in some. The recoveries of the mothers were more favourable than in former instances where no such agent was employed. In all, the children were born alive, except one, where the child was *acephalous*.

"To explain the small number of cases in which it has been

used here, I ought to mention that, in consequence of the deaths of two ladies in a respectable rank of life, from puerperal fever, when it was epidemic a few months ago—in both of which cases chloroform was used—the public, of course, attributed the unfortunate result to the new agent; and since then it has been difficult, and in many instances impossible, to overcome the prejudice against it. Of late, however, from the favourable opinion entertained of it by those who have had it administered, it is not so frequently objected to.

“Yours, &c.

“GEO. PATON.”

Dr. Anderson, president of the Medico-Chirurgical Society of Glasgow, and one of the most experienced and esteemed physicians in that city, some time ago wrote me a letter, from which I make the following extract:—

“Not being connected with any obstetric institution, and having now little practice of this kind amongst the poor, I am sorry I cannot offer you any sufficient statistics on the use of chloroform; but I have had so much reason to be satisfied with its effect that I almost always use it, and most of my patients have expressed themselves as strongly in its favour as you have done. I recollect only two exceptions, where, although the usual effects were produced, both patients complained that, previous to the full anæsthetic effects, their feelings, instead of being agreeable, were very much the reverse. One lady said, that she felt it like approaching insanity; and that she would rather endure the labour pains than be subjected to this again. In no case have I had reason to suppose that mother or child suffered injury. All the mothers have done well, and several of them have spontaneously remarked, that they made better and quicker recoveries than after former accouchements. In this I coincide. Two children were still-born. One, in the sixth month, died during parturition, after long-continued discharge of the liquor amnii for many weeks before the induction of labour; the other, in the eighth month, after hemorrhage from fatigue, followed by protracted labour: in this case a placental clot was found after delivery, so that the child's death was sufficiently accounted for.

“On the whole, my experience of chloroform in parturition is more favourable than in other cases, where I have several times seen it produce alarming spasmodic and other nervous symptoms, followed by sickness. These effects, I think, are most apt to occur in subjects who have had spinal irritation, or an excitable state of the nervous system, or who use the chlo-

roform when exhausted, or after abstinence from nourishing food.

Yours, &c.

"A. D. ANDERSON."

The following communication was sent me in June last—

By Mr. Spencer, Douglas, Isle of Man.

"I forget how often I had used the chloroform when I last wrote you; but, since then, I have used it frequently, with invariable success so far as it is concerned. There have been no still-born children from its use, and all the children have continued to thrive well; none as yet have had a fit from teething—this I consider a great matter. We want some powerful proof here to bring it into general use. I cannot use it in half my cases, as I would not do it without leave from the patient. I had a case of puerperal mania the other day, and did *not* use it in it. If I had used it, I should have been blamed. This is so small a place that everything one does is at once known. I have often operated under its use, with no ill effect.

"Yours, &c.

"J. H. F. SPENCER."

From Mr. Ceely, Aylesbury.

"I have much pleasure in communicating to you the general results of my very limited use of anæsthetic agents in midwifery. I have used both ether and chloroform in numerous operations, small and great, in public and private practice, have been several times the subject of their influence, and have repeatedly employed them in hysteria, epilepsy, and other nervous and spasmodic affections; but my employment of them in midwifery has been more limited, and at present restricted chiefly to primiparous and difficult or tedious cases. I have used them in only six cases as yet.

"It has happened that most of my late obstetric cases have been so easy and so rapid as to supersede the use of chloroform. My own observations of its effects in midwifery, would induce me to *urge* its use in all *severe, protracted, or operative* midwifery; and in any case of natural labour, with only the ordinary suffering, I would not hesitate to employ it, with the concurrence of the patient or friends, if I saw no contraindication; because I believe that, in proper doses, with suitable management, it is harmless and may be beneficial; but if the patient, under these circumstances, were averse, I should not, of course, press it. Neither do I think I should venture to employ it in natural and easy labour, where I suspected or knew that subsequent hemorrhage would arise, from flabbiness

of fibre and flaccidity of the organ; because I think the *secale cornutum*, during the last hour or two of such labours, has in my hands been so beneficial, and seems more appropriate.

In all the cases in which I have employed ether or chloroform, the patients and their friends have been perfectly satisfied and gratified with the results, and abundantly thankful for the boon. In only one case has there been any sickness or syncope. In a town about twelve miles hence, it has also been successfully used by Mr. Knight of Brill, in several cases. In Bicester, I have heard of its use, but chiefly by younger medical men.

"I will only add that, in using the chloroform, I have, in some of the cases, now and then omitted to repeat the dose, to show the friends and attendants how differently the patient was situated: but the latter has soon removed any hesitation of the friends by calling out hastily for that 'nice chloroform' again.

"Yours, &c.

"ROBERT CEELY."

In a printed pamphlet* obligingly furnished me some time ago by Mr. Stallard, surgeon to the Leicester General Dispensary, that gentleman observes:—

"I have exhibited the chloroform in upwards of thirty cases of midwifery during the present year, and they have included a greater number of severe cases than the general experience would support; this is accounted for by the fact that two of the worst were pauper patients, to whom the surgeon's attendance is required only in cases of unusual severity. One other formidable case also occurred to my father, and in it I was requested by him to use the chloroform. In this experience I have never once observed the least retardation of the parturient paroxysms, *so long as absolute insensibility was not induced*; and in several cases the pains were palpably increased. When a patient is suffering acute pain, she does all in her power to suppress it, and the act of doing so is well known to retard the process of parturition; when, therefore, the pain is no longer felt, the effort to suppress it no longer ensues, and the accouchement is accelerated in a remarkable degree. But again, it has been argued that the pain of labour is desirable; and that it is wrong to interfere with this natural indication of what is going on. Now, with this assertion I am totally at variance; there is nothing so depressing to the powers of life; nothing so soon exhausts the already feeble body; nothing so effectually prevents the sufferer from aiding nature by her volitional effort, as

* Practical Observations on the Administration and Effects of Chloroform in Natural Labour, p. 16, &c.

pain; and consequently nothing so powerfully maintains the strength, or invigorates the system, as its annihilation. In the thirty cases I have attended I have not had a single case of flooding, and two individuals had never been free from it on former occasions. This is, in my opinion, an additional recommendation for its exhibition; and in all those excitable persons in whom flooding is most likely to occur, the diminution of fear and excitement caused by its exhibition has a direct tendency to prevent the flooding. Nothing but a very extended experience can justify any conclusion upon this head; as far as my own experience goes, I am decidedly of opinion that chloroform exerts no perceptible influence upon the child.

From Dr. Protheroe Smith, London.

"I have records in my own practice and that of my friends of upwards of 125 cases of anæsthetic labour; and, with one exception, all have done well. In several thus treated, no hemorrhage has ensued, though in previous labours there was flooding. In nearly all, the getting up has been more speedy, requiring no aid of opiates and purgatives; and it is my sincere conviction that chloroform lessens the chance of puerperal inflammation and fever. I usually employ my inhaler to administer it, and with it I fancy I can administer it with greater success and precision. I have kept patients under its influence from half an hour to twenty-eight and a half hours. I have used it in cases in which bronchitis was present, and one lately, in which, at the time, there was complete aphonia. She has never had so good a time—in four or five previous births she had protracted recoveries, floodings, peritoneal attacks, &c.; these she has wholly escaped. I have used it in turning and forceps cases, &c., and in all I have been most successful. In administering chloroform I adopt your plan of inducing rapidly complete sleep, and afterwards keep up the effect by repeating the inhalations at each recurrence of uterine effort. The other day I had three cases, the one after the other, which strikingly illustrated the various effects of the chloroform on different subjects,—1st, a primipara, æt. twenty (administered in the second stage), kept under its influence four and a half hours, slept sweetly without any movement of the body, save during the last expulsive efforts, when the usual abdominal muscles were called into action. The "pains" under the chloroform became more frequent and powerful, and a fine male child was born. After the expulsion of the child, the patient awoke refreshed as from a sweet sleep, without any pain or evidence (save the advent of her first-born) of having been confined, as far as her own feelings were concerned. The uterus con-

tracted firmly, and I instantly left her for the second—a lady of title. She required to inhale for a minute and a half (in the third stage), before the induction of anæsthesia was complete, when she became very talkative, addressing her maid in French, speaking of matters wholly foreign to her condition, and of an amusing character, as though she was perfectly at ease and had nothing to do with the labour, which advanced rapidly, and terminated in a hour from the first exhibition of chloroform. On recovery, in about three minutes after, she was introduced to a fine boy, and remarked she had been sound asleep. The third case had the chloroform in the *second* stage; she slept soundly in half a minute, and then became conscious of what was going on—described to the nurse who stood by, the progress of the labour, and observed that she was greatly delighted that she had her senses, and yet was wholly unconscious of any pain or distress of any kind. She remarked that with the entire freedom from suffering she was sensible of the progress of the labour, as though it was effected by an agency with which she was altogether unconnected. During the last three I made her sleep; so she was ignorant of the birth of a fine boy: in this and the second case the placenta immediately followed; the abdomen and uterus contracted firmly. In all three cases no after treatment was required, and they make rapid recoveries.

“Yours, &c.

“PROTHEROE SMITH.”

From Dr. Rigby, London.

“Of course I use the chloroform in midwifery, but I cannot think that a large dose at first is the best way. In almost all private patients, you can draw the line between loss of pain and loss of consciousness. I give but a small quantity at a time; the patient takes it herself (on a strip of sponge), and when it is exhausted she asks for more.

“Yours, &c.

“E. RIGBY.”

From Mr. Lansdowne, Bristol.

“I have now used ether or chloroform in seventy-one midwifery cases; I have two modes of administering it, the one with a bladder, in which is placed a brass pipe with a stop-cock, and into this is screwed, after I have poured the chloroform into the bladder, a piece of elastic tubing with a mouth-piece, the whole being pierced with a bore $\frac{3}{8}$ of an inch, through which the vapour can be readily inhaled. If I find I am likely to be giving the chloroform for a long time, I use

this apparatus, both for the sake of convenience, and also of economy, as 3j. will last me nearly or quite an hour with this; and, should I use it many hours, it not only effects a great saving of material, but does not so frequently require replenishing, and is always ready at the approach of each separate uterine action; and it may (as has been the case with me) used by any friends, or by the nurse, should the practitioner require to be absent for a short time. The other apparatus is an inhaler, such as is commonly sold; it is made of a thin and pliable lead, adapted over the nose and mouth, having a piece of perforated zinc in its front, and containing a piece of sponge, over which the chloroform is thrown: the depth of this inhaler is such as to prevent the nose being touched by the chloroform. It is home manufactured, not expensive, and very easy of construction. This latter I make use of if I am likely to be wanting it for a short time only; it requires to be supplied afresh every five or ten minutes, and, accordingly, I use 3j or 3ss, which latter is my quantity when about to extract a tooth. If the action of the uterus causes great pain, as is frequently the case in an early period of the labour with the first child, I commence giving it as soon as the os uteri is sufficiently dilated for the head to pass; I have given it when the opening has not exceeded the size of half-a-crown. I believe it may be given with impunity as early in the labour as we please, and the only obstacles to its being so used that I can see, are the inconvenience to the medical attendant in being thus occupied with one patient for such a length of time, and also the very great expense which such a lengthened use of it must entail. On the patient's account, I can see no possible reason why it may not be used for a whole day, or even more; indeed, I cannot see why a limit should be set to the length of time in which it may be used. I have no doubt but that it will soon be the anodyne generally used at the latter stages of painful cancerous diseases. The greatest length of time in which I have used it has been 16½ hours, a fresh inhalation being made at every renewal of the action of the uterus; in other cases, I have given it 11½ and 12 hours, and the only reason of the inhalation being limited to this time, has been the cessation of the necessity for its use, namely, that the child has been born, otherwise it would have been continued until such event had taken place.

I have found that nearly all my patients have recovered very rapidly; most of those who have had children previously, have been astonished at the unusual rapidity of their recovery.

I find no difference as to the expulsion of the placenta and the subsequent discharge, when administering chloroform, to what takes place in the usual natural labour. I have, upon two oc-

casions, used it for very severe after pains, pains so severe that their cries could be heard at a considerable distance; indeed they appeared worse than the pains of actual labour; in both cases the pain was completely subdued by its use. Both these persons had determined not to avail themselves of the benefit of the chloroform during labour, neither did they, but they were delighted afterwards with its soothing effects. The former of these I had long resolved to give it to for this express purpose, it was her thirteenth child; her labours have always been very rapid, scarcely any pain accompanying them; but no sooner has the child been born than her agony has been almost past bearing, the pain recurring at intervals for a fortnight. Upon this occasion I gave it to her three times within the first ten hours, and she had nothing to complain of afterwards.

"As regards sickness, I have not found that symptom, except where fluid has been previously taken; on the contrary, if the patient has been sick the chloroform has almost invariably checked it. The cramp I have not heard them suffer from whilst under its influence. I have never yet met with anything which has caused me to regret having used it.

"Yours, &c.

"J. G. LANSDOWNE."

During the course of last winter and spring the practice of anæsthesia in midwifery was tried in London by a very intelligent young Russian physician, Dr. Haartman of Hessingsfors, while, for the sake of information, he was living as house-surgeon in the great Lying-in Hospital at Westminster. Dr. Haartman subsequently visited Edinburgh, and kindly drew up for me, before leaving England, the following account of the results of his practice. I have much pleasure in publishing his letter, both on account of its own intrinsic value, and because the results of the practice of anæsthesia in this Hospital have been in no small degree misrepresented, both publicly and privately.

Results of Anæsthesia in the Westminster Lying-in Hospital.

"I fulfil with the greatest pleasure your wish, in writing down my observations on chloroform as used in midwifery. I do it certainly not without much embarrassment, the greater the more I think of my ignorance of the English idiom; but, encouraged by your indulgence, I do not hesitate to begin the task, more especially as I know that some erroneous ideas of the use of the chloroform in the General Lying-in Hospital in London have been spread about.

"During the three months I was house surgeon in that hospital,

I had the opportunity of observing 105 women in labour, of whom about twenty-five were attended by my friend Dr. Delafield of New York, the other eighty by myself. But having been most of the time the only house surgeon, and otherwise much engaged, I was not able to exhibit the chloroform oftener than in twenty-five cases, which, I need scarcely say, were in no way selected. The most of these patients were, however, *primiparæ*. In the administration of the chloroform I followed your method, using simply a towel, upon the interior of which the liquid was diffused; but I held it as a rule to exhibit it in the beginning of each case rather in small doses, and with caution, watching, in the mean time, the pulse as the surest indicator of any danger. But when the patient got accustomed to it I gave it freely, not removing the towel till the patient was in deep sleep. In this state, the respiration was in general sonorous, sometimes stertorous; the pupils were usually somewhat contracted, sometimes, however, dilated, sometimes not at all affected. The pulse was, in the beginning, either a little accelerated, or quite natural, I think, when the patient was not frightened. Yet, I have seen the pulse without any bad consequence go down to 40, when the chloroform was used for a long time or in large doses. Its strength was very seldom altered, being then weaker. The patient usually lay quiet, all the muscles being relaxed till the uterine contractions came on, during which they all, with few exceptions, pressed down as in common labour. Some of them complained, although they afterwards said, 'they felt no pain whatever.' Few talked or laughed, except the Irishwomen, whom I found rather refractory to the anæsthetic influence, probably because they are, in general, too much accustomed to drinking spirits. All the cases were, with few exceptions, observed with the greatest care, and both the duration, the frequency, and the nature of each uterine contraction, both before and after the chloroform was exhibited, were marked down. Thus, I find, by comparing all my annotations, arranged in tables, that, during the full effect of the chloroform, the uterine contractions became less frequent and, I should say, less powerful, but, when the effect had passed off a little, they then became more frequent and shortened in duration; for example, if the interval between two uterine contractions before the use of the chloroform was 6 minutes, and the duration of each $1\frac{1}{2}$ minute, both these periods were reduced after its exhibition. The interval, from 6 to 5, 4, or 2 minutes, and the duration of each pain from $1\frac{1}{2}$ to 1 or $\frac{1}{2}$ minute. Besides, I think, that in this state the uterine contractions in general came on more suddenly than in common labour, a circumstance which, however, may depend on an illusion, by the

absence of pain existing and announcing the action of the uterus in the usual labour. I have never observed the uterine contractions quite arrested, yet often somewhat delayed. The supposed relaxation of the soft parts seems to me at least uncertain and accidental. In two cases in which I thought the chloroform had very much relaxed the rather rigid parts, I had, notwithstanding, rupture of the perineum. Among the twenty-five cases of chloroform, I had only in one a slight hemorrhage, a fact the more remarkable, as at the epoch at which I used chloroform most frequently, I can say I had hemorrhage, and that often very severe, in almost all the other cases in which chloroform was *not* used.

"After the delivery, I found the patient in general quite unconscious of any occurrence during the anæsthetic state, some of them saying, nevertheless, that they had felt something going on, although they did not know what. They seldom complained of headache, and it usually disappeared during the following day. The mothers recovered all speedily and perfectly, with one exception worthy your attention, although I am fully convinced that the accident by no means was the result of the chloroform, of which only two drachms were used—my provision at that time not being larger. During the use of the chloroform this patient was, as usual, insensible and quiet; but, for want of chloroform, the effect could not be kept up for a long time, and she was delivered with the usual pains. She was the following day perfectly well, and continued so till the eighth day after the delivery—she had then complained to the other patients of headache. I was, however, not called for before she, in the afternoon, had had a fit of what is commonly called apoplexia nervosa; after which she got paralytic on the right side. I need not to give you a tedious description of the proceeding, and the treatment of the disease; allow me only to state that the patient began, by the use of strengthening medicine, to walk about in a month.*

"Of all the 105 children, six were either still-born, or died sooner or later after the birth—two only of them belonging to the twenty-five cases of chloroform. But, before I relate these cases, I beg permission to point out some general remarks. In the majority of the cases I could not discover any change whatever upon the child, some of the children being rather bloodfull, others anemic—states depending, I think, as usual, on the du-

* The "puerperal paralysis" was in this, as it is in most other cases, probably connected with albuminuria. One of my patients, who was confined for the second time, four months since, was attacked two years ago with hemiplegia immediately after the birth of her first child. I have a patient at present under my care, from Forbes, who several years ago was attacked with hemiplegia at the time of delivery.

ration and the nature of the labour. In one case, however, in which Dr. Fergusson was present, the newly-born infant was found rather strange, or, to use the eminent doctor's expression, 'tipsy.' It breathed less frequently and more abruptly than usual, the sound of its scream being rather singular. It recovered, nevertheless, perfectly. I must here observe that the mother (Irish), who, after the confinement, had brought up a large quantity of gin and broom, confessed having taken spirits before admittance into the hospital. Of the two children who died, the first was rather weak and thin, but continued pretty well till the seventh day, when it got peritonitis, and died on the tenth day after birth. The second, a large and fat child, died four hours after birth: in this case, when the head was born, I observed around the neck small blue spots, which I found to be blood extravasated under the skin. After birth the same kind of spots began to appear over nearly the whole of the body. The child breathed well in the beginning, but would not suck. It died without convulsions. By the *post mortem* examination, the spots were found to be blood, extravasated from the capillary vessels of the skin; a large quantity of half-coagulated blood was found in the cavity of the peritoneum, and small blood coagula in both lateral ventricles of the brain. The lungs and the brain were slightly congested. The blood seemed to be quite natural, and had no anormal smell.*

"Before I finish, I beg to add that I have used the chloroform in a case of turning with the greatest advantage. I was called out from the hospital to a poor woman in labour, and found a presentation of the cord and the uterus so contracted that I could nowhere introduce my hand, although I made repeated attempts without success. I then gave a large quantity of chloroform, and I was quite astonished at the great change which took place, it being now uncommonly easy to perform the turning.

Yours, &c.

"CHARLES DE HAARTMAN."

Of the use of chloroform in midwifery in Ireland, I have few details. From Dublin, my friend Dr. Tyler wrote me in June last, that the practice has not yet been much tried. He observes:—

* This special case of purpura has been often mentioned. Every one acquainted with infantile pathology knows that purpura is not a rare occurrence at birth, as the cases and observations of Andral, Billard, Otto, Lobstein, and Graetzer, &c., amply prove. A patient who came some time ago from London to be confined in Edinburgh, under my care, was gravely assured by a distinguished London physician, that if chloroformed, her child would be sure to be, as they all were in the Westminster Hospital, "either dead born, or with their blood in a putrid and dissolved state!" The want of the head in the anencephalic child born at Dundee (see Dr. Paton's letter, p. 166), might have been as logically ascribed to the mother's use of chloroform, as the purpura in the above instance.

"As to the progress of chloroform in obstetric practice here, I regret to state that its virtues have not yet been fairly put to the test by any of our Dublin accoucheurs, owing to a dread of bad consequences resulting, although I am unaware of any case followed by such being brought home to it, except a rumoured one of *threatened* convulsions and spasms, where ——— was administering it.

"As to the surgeons, I hear them all speak in the highest terms of it. The resident surgeon of Steven's Hospital, Mr. Wilmot, told me yesterday that he administers it previously to every operation there, now in some hundred cases, without meeting with any untoward result.

"Yours, &c.

"A. TYLER."

Subsequently Dr. Tyler informed me that Mr. Shekleton, master of the great Dublin Lying-in Hospital, was trying the effects of chloroform. While this sheet was passing through the press, Dr. Denham, assistant physician in that hospital, visited Edinburgh, and informed me that Mr. Shekleton has now exhibited chloroform in upwards of forty cases of labour, most of them of an operative or instrumental kind, and that the effects have been such as to induce him to go on with the trial of it. No unfavourable results, either as regards the mothers or children, have been observed to follow the use of chloroform. Some of the principal private practitioners in Dublin, are also, Dr. Denham informs me, beginning to employ the practice.

Regarding the use of chloroform in midwifery on the Continent of Europe, I possess little or no information. Dr. Krieger of Berlin, tells me that in that city most medical men are opposed to the practice of anæsthetic midwifery, but still, five or six accoucheurs constantly make use of it. Writing to me in July last, Dr. Krieger says:—

"About five or six accoucheurs of this place, I don't think there will be more of them, use chloroform in almost every case they attend; many more only in cases of morbid labour; the majority not at all. The cause of this curious occurrence—*curious*, because in *every* surgical operation chloroform is made use of—may be found, not only in the disinclination of the public at large to such extraordinary means in quite a natural process (as they take labour for), but perhaps also in the expense, chloroform being still a costly article. I don't know whether the preparation we get here is less strong than it ought to be, or not, but I seldom require less than six drachms or an ounce, sometimes more, for one delivery, and the price is as much as half-a-crown per ounce at apothecaries' shops. Pro-

fessor Martin of Jena has used ether in seven, chloroform in ten cases of morbid labour, and cannot strongly enough recommend the latter anæsthetic agent, stating, that he never witnessed but favourable consequences. I am sorry I cannot give you any more details about the progress chloroform has made in Germany—but the perplexing political affairs have so very bad an influence on scientific publications, that we get but very little medical news from anywhere on the Continent, and those only such as were long prepared before we fell victims to revolutions.

“Yours, &c.

“E. KRIEGER.”

In a letter which I have lately received from Vienna, the writer, Dr. Arneth, informs me that the use of anæsthetic agents in midwifery is, as a general rule, discountenanced in that city by the two leading professors of midwifery in the University, but that, in several late cases, they have used it in operative delivery. He further states, that in Würzburg the use of chloroform in obstetric practice is more common. He states—

“While I am writing this letter, Professor Kiwisch, of Würzburg tells me, that in that place no woman is confined, neither in private nor in hospital practice, without having been chloroformed. In two cases of eclampsy he saw decided effect of these vapours, viz., the fits subsided.

“Yours, &c.,

“F. H. ARNETH.”

The preceding kind of evidence, in relation to the practice of anæsthesia in midwifery in this and other countries, might, as I have already stated, be very easily increased and multiplied by a little more extended inquiry, and by an appeal to the experience of the numerous accoucheurs here and elsewhere that have employed it. But the evidence as it stands—and spontaneous as it is in most cases—is amply sufficient to show both the great extent to which the new practice has already been adopted, and the great success that has attended upon it. Every innovation in medicine which implies, like the present, a violent and extensive change in existing doctrines and old-established practices, has always been, for a length of time after its introduction, stoutly decried and resisted. The history of the first introduction and subsequent progress of the three greatest modern improvements in practical surgery, midwifery, and medicine—viz., the ligature of arteries, the induction of premature labour, and the discovery of vaccination—afford

sad but strong historical proof of this observation ; and we have many minor instances of the same constant enmity to change, in the bitter opposition which the first employment of antimony, ipecacuanha, cinchona bark,* and other medicines encountered. And I believe that I am correct in stating, that probably no innovation, embodying so very direct and decided a deviation from all the former routine and rules of practice, as the employment of anæsthesia in midwifery implies, ever, in the same short period, made such extensive way and progress as it has done among the profession. As a matter of course, however, it has called forth also abundance of published and unpublished opposition and objection. No small share of the resistance against it has taken the form of personal or professional abuse of me as the introducer of the practice. All that I most willingly pass over and excuse, as, judging from all past experience in medicine, it was nothing more nor less than I was entitled to expect under the circumstances. But some difficulties and objections of a more palpable nature have been urged against the practice ; and, in conclusion, I will very briefly allude to, and attempt to answer, the more prominent among these that happen to have been brought forward.

* The London physicians have, on several occasions, specially distinguished themselves by their determined and prejudiced opposition to all innovations in practice not originating among themselves. In the whole Pharmacopœia, there is perhaps no one remedy which, at the present day, is acknowledged to be of greater value, or to have saved more human lives, than cinchona and its preparations. In the seventeenth century, the proper time and manner of using the cinchona bark, for the cure of the then prevalent intermittent fevers of England, was made out by Robert Talbor, a medical practitioner in Essex. When Talbor subsequently removed to London, and began to use with success the new remedy in the cure of the common agues of the metropolis, he found that, as he gained the favour of the world, he lost that of the physicians of London ; and apparently their persecution of him became such that the King at last was obliged to interfere, and in the year 1678, King Charles II., sent a royal mandate to the College of Physicians, commanding the president, Dr. Micklethwait, "and the rest of the College of Physicians," not to give Talbor "molestation or disturbance in his practice." Among the list of London physicians averse to the new practice of curing ague by cinchona bark, De Bergen mentions the illustrious names of Sydenham, Harvey, &c. In 1698, a Dutch physician, Dr. Groenvelt, published a work entitled, "De tuto cantharidis in medicina usu interno." A few years previously, viz., in 1693, when Groenvelt practised in London, the President of the College of Physicians imprisoned him in Newgate for daring to recommend and use the new remedy whose virtues he had discovered. Six or seven years after vaccination began to be generally used throughout England, Dr. Moseley, a member of the London College of Physicians, suggested to his College the propriety of putting down "the beastly new disease," as it was termed, of cow-pox ; and in 1805, he boasted that the middle and inferior classes of London had then "renounced the delusion." In the last number of a respectable London medical journal, a London medical practitioner questions whether the practice of relieving women, by anæsthetics, from the pains and agonies of parturition, should not "be considered criminal according to law !" See *London Medical Gazette* for Sept. 8, p. 424.

CHAPTER IX.

ALLEGED DIFFICULTIES IN THE SUPERINDUCTION OF ANÆSTHESIA.

IN a previous page, I have stated the principal circumstances which require to be attended to in the exhibition of chloroform in labour. I have been occasionally told of cases, in which it was supposed that it was impossible to produce the anæsthetic effect of this agent. In my own practice (and I have now used chloroform in many hundred persons), I never yet met with a single instance in which a person was proof against its full influence.

It has been sometimes averred in the English journals, that on attempting to use chloroform, jactitation, incoherent talking and delirium, spasms, &c., &c., have supervened *instead* of a state of quiet anæsthesia. These are symptoms which do occasionally come on in the *first* or exciting stage of its action, more especially if strict quietude is not enjoined; and, though they are apt to terrify the beginner, they are in reality no more serious in their effects and character, than some of the equally frightful symptoms sometimes seen in hysteria. They are an evidence, however, of one of two things; either that the vapour is being given too slowly, or that it is given in too small quantity,—in an exciting, in fact, instead of a soporific dose; and the simple remedy, as every one properly experienced in its action knows, is at once to increase the dose in order to pass the patient as speedily as possible into the *second* or full narcotic stage.

Chloroform, it is alleged, sometimes gives rise to much coughing, and pulmonary irritation. Certainly not so, if the chloroform is of good quality, and its vapour is not at first approached in too strong and concentrated a form to the face of the patient. After some experience, it will be found that it can be given so as seldom or never to induce even coughing. Some time ago, a well-known physician, in a large city of the south of England, wrote me, that he and his townsmen had found it too dangerously irritant a substance to breathe, and that he had seen it produce cough, bronchitis, phthisis, &c. The answer was simple; it never produced any such effects in Edinburgh practice. And I believe that the explanation was

equally simple; he and his townsmen had experimented with an imperfect and impure article. A few days ago, one of the principal druggists in Edinburgh showed Dr. Christison, Dr. Douglas Maclagan, and myself, a bottle of chloroform of high specific gravity, viz., 1490, which he had just received from a very large manufacturing chemical house in London. It was impossible to breathe it without feeling great irritation in the throat and chest. It emitted fumes that at once reddened litmus paper; and which, on examination, proved to be muriatic acid. Is it wonderful that bronchitis, coughs, and more serious disasters, should have followed the inhalation of such an improper and dangerous article?

Dr. Letheby has shown, that some kinds of chloroform in the market, besides containing muriatic acid, are also mixed with aldehyde, hydrochloric ether, hypochlorous acid, &c.*

* See *Medical Gazette* for June 16, 1848, p. 1038. The presence of some of these deleterious agents has been supposed to be an inevitable and speedy effect of the spontaneous decomposition of very pure chloroform. But I find that some of the article, manufactured several months ago in its purest form by Duncan, Flockhart, & Co., of this city, has undergone no kind of change, even though long exposed to the sun. Messrs. Smith have also shown me the same, in regard to their chloroform. I have reason to know, that the dangerous article alluded to in the text as containing a quantity of muriatic acid, has been extensively sold to the profession, throughout Scotland and England, at a price two or three shillings per pound cheaper than is charged for the pure chloroform manufactured by other houses; and, probably, its very cheapness has led to its extensive use. The following is the formula by which chloroform is prepared by Messrs. Duncan, Flockhart, & Co., of Edinburgh, whose article I have always found of the most superior quality:—4 pounds of chloride of lime, and 12 pounds of water, are first well mixed together, and then 12 ounces of spirit added. Heat is then applied to the still (which ought not to be more than a third full), but as soon as the upper part of the still becomes warm, the heat is withdrawn, and the action allowed to go on of itself. In a short time the distillation commences, and whenever it begins to go on slowly, the heat is again applied. The fluid which passes over separates into two layers, the lower of which is chloroform. This, after having been separated from the weak spirit forming the upper layer, is mixed with half its measure of strong sulphuric acid, added gradually. The mixture, when cool, is poured into a leaden retort, and distilled from as much carbonate of baryta by weight, as there is of sulphuric acid by measure. The product should be allowed to stand over quicklime for a day or two, and repeatedly shaken, and then redistilled from the lime. The specific gravity of the resulting chloroform is generally 14·96 or 14·97.

CHAPTER X.

OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.

OBJECTIONS of various kinds, religious, moral, and medical, have been zealously brought against the practice of anæsthesia in midwifery.

Elsewhere* I have attempted to answer the supposed religious objections that were at first so very strongly urged in various quarters against the practice, on the supposed ground of the permanence of the primeval curse; and I have shown that the disputed word "sorrow," *'etzeb* ("in sorrow thou shalt bring forth children"), does not in the original Hebrew really signify the sensations of pain, but the muscular efforts and contractions connected with childbirth. Besides, if this were not the fact, and that it was the duty of man to give effect to the curse, instead of struggling to ameliorate and resist its penalties and influences, then the whole art of physic should require to be abandoned entirely, for, in the primeval curse, man was doomed to die; and yet is not the great leading aim and object of the physician a continuous attempt to preserve him in life? All forms of obstetric assistance would require also to be rejected, for the whole art and science of midwifery is one undivided effort to abate and ameliorate the effects of the curse; and to attain that object imperfectly, as heretofore, by venesection, baths, by counter-pressure to the back, and other minor practices, is as sinful as to attain it more perfectly now by anæsthetics, inasmuch as the principle of interference is not altered by the degree of relief given; "for whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." In short, if there is any evidence of feelings of impiety and irreligion in the whole question, it is surely on the side of those persons who suppose that pain is permanently ordained in the primal curse as an accompaniment of human parturition; and yet that by anæsthetics, man, the creature, has discovered a power by which he can alter and subvert an immutable decree of God, the Creator.

The principal moral "objection," as it has been termed,

* See also Dr. Protheroe Smith's late pamphlet entitled, "Scriptural Authority for the Mitigation of the Pains of Labour."

against the employment of anæsthesia in midwifery, amounts to the often-repeated allegation, that it is "unnatural." "Parturition," it is avowed, is a "natural function," the pain attendant upon it is a "physiological pain" (Dr. Meigs),* and it is argued that it is impossible "to intermeddle with a natural function;" and to use anæsthetics is a piece of "unnecessary interference with the providentially arranged process of healthy labour" (Dr. Ashwell).† The above is, perhaps, the most general and approved of all the objections entertained and urged at this moment against the practice of anæsthesia in midwifery. But it certainly is a very untenable objection; for, if it were urged against any of our similar interferences with the other physiological functions of the body (every one of which is as "providentially arranged" as the function of parturition), then the present state of society would require to be altogether changed and revolutionized. For the fact is, that almost all the habits and practices of civilized life are as "unnatural," and as direct interferences with our various "providentially arranged" functions, as the exhibition of anæsthetics during labour. Progression upon our own two lower extremities is a "providentially arranged" function, a "natural process;" and yet we "unnaturally" supplement and assist it by constantly riding on horseback and in carriages, &c. The "physiological process" of walking is apt to produce pain and injury of the uncovered foot of man, and we "unnaturally" use boots and shoes to bind the foot, and add to the protecting power of the cutaneous and other structures of the sole. Mastication and digestion are "natural processes;" but we daily intermeddle with and attempt to aid them by the arts of cookery and dietetics; and so on with regard to other functions.

To annul the pain of labour by anæsthetics is, argues Dr. Meigs, "a questionable attempt to abrogate one of the general conditions of man." Riding and railway travelling abrogate one of the general conditions of man (progression), and are constantly leading to accidents and deaths. Should we never travel therefore except on foot? Disease and death itself form one of the most "general conditions of man,"—and medicine is a "questionable attempt to abrogate them." Should medicine therefore be abandoned?

In a note now lying before me, an eminent London divine urges the following objections against anæsthesia either in midwifery or surgery; and I notice it here, because it is an objection which I have often heard repeated. He writes:—"The question with me is not the alleviation of pain, but the

* Philadelphia Medical Examiner, March, 1848, p. 152.

† Lancet for March 11, 1848, p. 291.

destruction of consciousness. I should hesitate greatly to take a step which destroys consciousness." Now, certainly, our consciousness is destroyed in natural sleep as much as in the anæsthetic sleep. I have little doubt that the distinguished writer whom I have quoted, has, many a day, perhaps during almost every day for a long lifetime, voluntarily given up and destroyed his own consciousness in sleep, for an hour or two longer each morning than the necessities of his system required. Putting these many hours together, he has perhaps now, from first to last, unnecessarily but voluntarily, surrendered up his mental consciousness for periods that, if added together, would count up weeks, and months, and perhaps years. He has done so too, merely for the reprehensible indulgence of indolence; and yet he insists upon his fellow-creatures not surrendering up *their* consciousness for a short time on rare and extraordinary occasions, when the object is the far more legitimate one of the avoidance of unnecessary physical pains, and the securing life and health by saving the system from the endurance of these pains. If we may sleep, and thus indulge in the destruction of consciousness to avoid and cure corporeal fatigue, surely we may do the same to avoid and cure corporeal agony.

Dr. Merriman* opposes the employment of anæsthesia in natural labour, on the ground of "the great superiority of allowing nature to conduct the whole process of the birth." But the practice of anæsthesia does, in reality, allow "nature to conduct the *whole* process of the birth:" it merely abstracts that intensity of pain and suffering which accompanies the act of labour in the civilized woman,—a "disadvantage inseparable" from civilization, to employ Dr. Merriman's own expression, and which is not an essential part of the process of parturition, according to his own doctrine; for, as he himself states, "in the earliest ages of the world, and in savage nations at present, childbirth appears to have been, in almost every instance, *easily* accomplished; the mother suffers *little*." And in this state of natural anæsthesia the convalescence of the mother is consequently unusually rapid; for again, to quote Dr. Merriman's words, she almost "at once resumes her ordinary occupations." Dr. Merriman afterwards, in speaking of the use of chloroform, decries its propriety in any except "instrumental or very tedious labours,"—arguing that we should not interfere unless where the labour is morbid, for (to use his own words), "the duty of the physician is to *imitate* nature as far as possible, and watch her methods of acting." But surely the

* Arguments against the Indiscriminate Employment of Anæsthetic Agents in Midwifery. London, 1848.

physician strictly imitates nature in her most natural state, according to Dr. Merriman's own premises, when, during labour, he induces by art that state of anæsthesia, which, in Dr. Merriman's opinion, *originally* pertains to parturition in the human mother. The female in an uncivilized state more truly shows us the true method and types of nature, than the female in a civilized state. Besides, are we not called upon to relieve the woman, when we can, of her sufferings, as an act of common professional duty and common professional humanity? In law and in morals, we judge greatly of actions by their intent. No accoucheur would intentionally inflict upon a patient the agonies of labour by a deliberate act of *commission* on his part. Is an accoucheur properly justified in intentionally refusing to save a patient from the agonies of labour by a deliberate act of *omission* on his part? When a child, at birth, is intentionally destroyed by the loss of blood, it does not matter, in the eye of the law, whether the death has been produced by voluntarily *omitting* to tie the umbilical vessels, or voluntarily *opening* other vessels.

Up to within the last few months, and till the power of annulling pain by the inhalation of ether was discovered, severe bodily pain, such as we witness in surgery and midwifery, was universally regarded by the profession as possessing an evil and morbid effect. Some of the opponents of anæsthesia have taken up a novel and different view; and, as a medical argument against the practice of anæsthesia in midwifery, it has been particularly averred that a labour pain is "a desirable, salutary, and conservative manifestation of life force," (Dr. Meigs.) Parturient "pain is (says Dr. Copland)* often salutary as respects its effects; neither its violence nor its continuance is productive of injury to the constitution," &c. No opinion, I believe, could be more erroneous. I have already shown, from the evidence of extensive statistical returns, that some of the graver operations of surgery are now much less fatal in their results when patients are operated on under the condition of anæsthesia, and consequently without any attendant pain, than the same operations were formerly, when patients were submitted to all the agonies of the surgeon's knife in their usual waking and sensitive state. The prevention of the pain in surgical operations is, in other words, one means of preventing danger and death to those operated on; the saving of human suffering implies the saving of human life. And what holds good in relation to pain in surgery, holds good in relation to midwifery. Pain, whenever it is great in degree or great in

* Dictionary of Practical Medicine, vol. iii. p. 484.

duration, is in itself deleterious; and by shielding our patients, by anæsthetic measures, against the more severe portion of the pains of parturition, we not only preserve them from the agony of their more immediate sufferings, but we preserve their constitutions also from the effects and consequences of these sufferings. And the evidence which I have adduced in the preceding pages tends to prove, that, when thus freed from the endurance of pain by artificial anæsthesia, they assuredly, as a general rule, make both more rapid and more perfect recoveries than when such means are not used; just as woman in a savage state, and where she enjoys a kind of natural anæsthesia during labour, recovers more easily and rapidly from the shock of labour than the civilized female. In short, in cancelling the pains of parturition by anæsthesia, we also, I believe, to a great extent, cancel the perils of the process; for the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital, when under Dr. Collins's able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, 22 died; *or 1 in every 320*. In 452 of his cases, the labour was prolonged above 20 hours; and of these 452, 42 died; *or 1 in every 11*,—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

The last and certainly the principal objection against anæsthesia in midwifery, is the supposed danger accompanying the exhibition of anæsthetic agents. In the earliest paper which I published on the subject of chloroform, I pointed out this circumstance in mentioning various cautions in the use of it. When we consider the immense extent to which it has already been employed in all quarters of the world, in medicine, surgery, and midwifery,—the little care sometimes observed in its use,—and the deleterious and dangerous articles with which, as we have seen (p. 181), it is sometimes mixed, the wonder is that so few alleged accidents have happened from its employment. By saving a vast amount of human suffering, it has already, I believe, been the means of saving no small amount of human life; and it is assuredly improper to argue, as some have done, that the mere chance of its disagreeing with some rare and special constitution, now and then, is any valid reason for refusing its use for the abatement and abrogation of human suffering. If there were any soundness in the reasoning, a thousand things beside would require to be abandoned. Railways, steamboats, stage-coaches, &c.,

when used as substitutes for the natural and physiological function of human progression, are ever and anon attended with accidents to limb and life. But surely no one would, from this, maintain that these means of conveyance should, in consequence be abandoned. Many persons are annually drowned in bathing.—Should bathing, therefore, be prohibited, and this powerful means of maintaining and restoring health be entirely forsaken? According to the Registrar's official returns, a great number of lives* are lost yearly in England by the improper medical use of opium.—Should the use of opium, therefore, be given up? Patients sometimes sink under the depressing action of antimony, calomel, &c.—Should these valuable drugs, therefore, be banished from the Pharmacopœia? Many a patient has perished in consequence of venesection.—Should this operation be expunged from the art of surgery? From mistakes and errors, &c., in diagnosis and practice, medicine and surgery are sometimes the unhappy means of destroying instead of saving life.—Should these arts, consequently, be interdicted? Works on medical subjects have sometimes led both patients and practitioners into serious and fatal errors.—Should no medical works, therefore, be allowed to be printed? Long ago Raynalde, in sending forth the first work on midwifery ever published in the English language, seems to have foreseen that, against the utility of publishing any book or books on midwifery, the same argument would be used as we have found in our own day used against the application of anæsthesia to midwifery; and he has answered the argument in a style so earnest and apposite, that I shall quote this reply, by anticipation as it were, in his own words, in an abridged form:—

“Lo!—such is the light judgment of them, the which in everything, whereof may ensue both good and evill, have alwaies their eyes waking and firmly affixed and directed upon the evill, picking and choosing out the worst of every matter, omitting and leaving to speake of the best, as the thing which were nothing to the purpose. If everything in this world should be weighed and passed upon after this sort, then should we be faine to condemne and banishe those things farre from us, which are at this time, accompted and taken for the most necessary, worthie, and of greatest price or estimation. To be short, there is nothing under Heaven so good but that it may be perverted and turned to an evill use, by them that be evill and naught themselves, and so abuse it: ne is there anything so absolute and perfect, but by the occasion of the abuse thereof, at one time or other may and doth ensue great danger and damage to mankinde? Fire and water be two right necessary elements to the use of man, without the whiche he could not live: yet by the meanes of them many a miserable deed hath been done and perpetrated. By fire hath bin consumed and devoured whole cities and countries; by water,

* In 1840, out of every 1,000,000 living in England and Wales, 24 were poisoned by opium, and 22 by other medicines improperly given to children below the age of five years alone.—(See *Seventh Annual Report*, p. 82.) See Taylor on Poisons, p. 187, &c., for the great numbers destroyed in England by opium, &c., improperly given.

swallowed and drowned infinite men, shippes, yea, and whole regions. Againe (he continues), meate and drinke, to the moderate users thereof doth minister and maintain life; and contrary, to the unmeasurable and unsaciate gourmands and gluttons it hath full many times brought surfeet, sicknesse, and at the last, death. * * * But (he argues) should men, for the avoyding of the aforesaid inconveniences, and for the reasons above said, condemne and banishe fire and water, or forsake their meate and drinke? No, it were but madnesse once to think it. Therefore I say, the judgment of that eye can never be equall and indifferent, which hath more respect and regard alwaies to the displeasures and hurts possible to happen (only through the misuse of a thing) than to the emoluments and profites daily and commonly like to ensue to the wel users of the same: and that of itselfe is good, is never to be disallowed for the sake of them that do abuse it. For to them that love good themselves, whatever it be, it is to them a sufficient matter and occasion therein to seeke the glory of God, and the only profit of their fellow Christian: And contrary, such as be of ill disposition, will in every thing, be it never so good and salutary, pick out matter of maintenance to their lewdnesse, turning matters of discretion to foolish and peevish prating contention."—(*Prologue to Raynalde's Birthe of Mankinde*, Edition of 1626, p. 9.)

DISCUSSION ON THE EMPLOYMENT OF CHLOROFORM IN MIDWIFERY AND SURGERY BEFORE THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

[Abridged from the *Monthly Journal of Medical Science* for July, 1848, p. 54.]

Dr. Simpson gave a long Report and detailed communication on the employment of chloroform in midwifery, stating that he had used it constantly, and with the best results, in his own practice since November; mentioning the rules required to be attended to in its exhibition; answering the supposed objections to its use, &c., &c. He read numerous communications and reports regarding its employment, from various practitioners, showing that a great number of persons had been already successfully delivered without pain or suffering under the use of chloroform during the last six months.

Drs. Moir, Malcolm, Keith, Carmichael, &c., stated to the Society some verbally, and others in writing, the uniform and successful results which they had met with, employing it, as they did, constantly in their practice, and in all cases of labour.

Mr. Crisp of London stated, that though a stranger, he was induced to rise, were it for no other purpose than to say, that, after having attended the meetings of many a medical society, he had never till to-night seen one that was *unanimous* in opinion on any topic. He had come to Edinburgh a fortnight ago, and now entertained a totally different opinion about chloroform from what he did when he arrived; for he had now seen it constantly and most successfully employed in the hospital and elsewhere. At the same time, although this had been the result of additional experience on his own mind, he was not disposed to blame, but, on the contrary, to commend the scepticism which had been shown by many most eminent men in London and elsewhere, on this subject, which he thought was no more than justified in relation to an agent of such a novel kind, and so important in its practical application. He believed that this scepticism had not its origin in any exclusive or bigoted feeling, but would be overcome as soon as the facts came to be as well known in London as they are in Edinburgh.

Dr. Bennett considered it probable, that one of the reasons chloroform was not much used out of Edinburgh, was the impurity of the article administered. It was not long ago that *Dr. Clay* of Manchester had stated to the Society, that although he had frequently seen it given in that town, he had never witnessed its

proper effects produced until he came to Edinburgh. Mr. Crisp from London had just made a similar statement. Dr. G. Wilson had lately informed him, that even the chloroform manufactured in Edinburgh was not so pure as it might be, and that he had lately purified some which produced the full effect more rapidly, and with a smaller quantity, than in ordinary use. He (Dr. B.) conceived that the purity of the chloroform was not sufficiently attended to by those who had tried it, and that those who would not, had better, like Dr. Clay and Mr. Crisp, come and see it given in Edinburgh.

Dr. Simpson observed, that he believed the want of success in England was owing also to another cause. From what he had learned, he was quite convinced that our English brethren, in using chloroform, often stopped altogether at that point which really constituted the true commencement of the effects of the inhalation. Immediately before the chloroform produced anæsthesia, more especially if there was any noise or disturbance, it not unfrequently excited the patient, who would talk incoherently for a moment or two, beg the inhalation to be suspended, perhaps struggle to get free from it, and have his arms and legs thrown into a state of strong clonic spasms. In Edinburgh, we all sufficiently know that these symptoms indicate merely that the patient is about to come under the full influence of the vapour, and that, in a minute or so, these symptoms will pass, and he will immediately be completely anæsthetic and completely unconscious. But in England these premonitory symptoms seem to have been often regarded as very alarming, and all attempts at further inhalations stopt, exactly where and when the dose of the vapour should have been increased. And in the English Journals such cases have been repeatedly and gravely recorded as instances of delirium, and spasms, and convulsions, and failure. They are not more anxious, or deserving of attention, than the same symptoms would be in a case of hysteria, and are quite transient if the inhalation is only persevered in. *Dr. Simpson* added, that now, amongst many hundred patients, he had never yet met with one instance in which any person was unsusceptible of the full effects of the chloroform. He knew that the experience of many of his brethren around him went to the same effect. Nor, in any one case, had he seen any marked bad effects from the fullest use of the chloroform. *Dr. Simpson* asked *Professor Miller* and *Dr. Duncan* to state the extent to which they used chloroform in the Royal Infirmary, and in their private surgical practice.

Professor Miller observed, that in the Hospital and elsewhere the surgeons of Edinburgh had used chloroform in all their operations, with the exception, perhaps, of any such within the cavity of the mouth as were expected to be attended with much hemorrhage. And he could speak of its perfect success, and perfect certainty, and perfect safety, in the most unequivocal terms. There had been no misadventures, no failures, and now no fears of those spasms and other preliminary symptoms to which *Dr. Simpson* had alluded. In saying all this, he believed he was simply stating the opinion and experience of all his surgical brethren here; and that no one amongst them would deem himself justified, morally or professionally, in now cutting and operating upon a patient in a waking and sensitive state. Every professional principle, nay, the common principles of humanity forbid it, seeing that surgery was now happily possessed of sure and safe means by which it could avoid the necessity of such cruelty. These were strong opinions, strongly expressed, but, in answer to *Dr. Simpson's* question, it was impossible for him to say less.

Dr. Duncan stated, that he sincerely coincided in every part of the statement made by *Professor Miller*, and that, in his Hospital and in his private practice, he constantly, like his other surgical brethren, used chloroform in all his operations, and even when making any painful examination for the purpose of diagnosis.

P A R T I I I .

ON THE NATURE AND POWERS OF VARIOUS ANÆSTHETIC AGENTS.*

CHAPTER I.

HISTORICAL RESEARCHES REGARDING THE SUPERINDUCTION OF INSENSIBILITY TO PAIN IN SURGICAL OPERATIONS.

DR. SIMPSON first entered at some length into the chemical and therapeutic history of sulphuric ether. He traced it from Lully, Hollander, Valentine, &c., in the 13th and 14th centuries, down to Valerius Cordus, who described its formation accurately in the 16th century; and Frobenius, who first designated it ether, and published an account of it in the Philosophical Transactions of 1730. He then pointed out its therapeutic history, from the first mention of it as a medical agent by Valerius Cordus, down to the works of Hoffman, Cullen, Alston, Lewis, Monro, and the other writers on the materia medica during the last century. The history of its use by inhalation commences with the pamphlet of Richard Pearson, published upon it in 1795. Since his time most therapeutic authors mention, more or less fully, its employment by inhalation in asthma, &c., as Duncan, Murray, Brande, Christison, Thomson, Pereira, Nysten, Barbier, Wendt, Vogt, Sundelin, &c. Its power of producing, by inhalation, effects like intoxication, or like the influence of nitrous oxide gas, he showed to have been stated by various American authors, as by Godman (1822), Mitchell (1832), Professor Samuel Jackson (1833), Wood and Bache (1834), Miller (1846), before it was so fortunately adopted by Dr. Morton as an anæsthetic agent. His belief was, that Professor Charles Jackson improperly claimed the merit pertaining to its recent happy application to surgery, &c. Perhaps the idea of relieving patients from the pains of surgery by some

* From the Edinburgh Monthly Journal of Medical Science, for Dec. 1847, p. 451.

such means, or rather the restoration of that idea in recent times (for it was an old one), belonged justly to Horace Wells.

Dr. Simpson then proceeded to show, that the idea was not one entirely of our own times, by taking a retrospective glance at the history of *painless* operations. Among external measures proposed with this view, he mentioned, 1. the idea of Moore (1784), to compress and obtund previously the nerves of the implicated part; an idea, however, which, long before Moore's time, was suggested by Ambrose Paré; and 2. the alleged ligation or compression of the larger vessels of the neck mentioned by Hoffman, Valverdi, Morgagni, &c.

Dr. S. then showed that various *internal* agents, introduced by inhalation and otherwise, had been at different times suggested and employed; as 1. In 1828, the previous inspiration of carbonic acid (?) gas proposed by Dr. Hickman; 2. In 1800, the hint of Davy regarding the application of nitrous oxide for this purpose; 3. The inhalation of vapour arising from the watery extract of mandragora, opium, &c. He read at length the following remarkable passage, (which he had found in the old surgical treatise of Theodoric, who died in 1298,) as an odd forestalling of the practices of the present day. In the rubric of the work the receipt is described as "*Spongia somnifera*," and described thus:—

"The preparation of a scent for performing surgical operations, according to Master Hugo; it is made thus:—Take of opium and of the juice of the unripe mulberry, of hyoscyamus, of the juice of the hemlock, of the juice of the leaves of mandragore, of the juice of the woody ivy, of the juice of the forest mulberry, of the seeds of lettuce, of the seed of the burdock which has large and round apples, and of the water hemlock, each 3 i; mix the whole of these together in a brazen vessel, and then in it place a new sponge, and let the whole boil; and as long as the sun on the dog-days, till it (the sponge) consumes it all, and let it be boiled away in it [or, in other words, let a watery extract be so formed]. As often as there is need of it, place this same sponge into warm water for one hour, and let it be applied to the nostrils till he who is to be operated on (*qui incidendus est*) has fallen asleep, and in this state let the operation be performed [*et sic fiat chirurgia*]. When this is finished, in order to rouse him, place another sponge dipped in vinegar frequently to his nose; or let the juice of the roots of fenigreek be squirted into his nostrils. Presently he awakens."

The above remarkable anæsthetic means was recommended in the thirteenth century. But mandragore was used even much earlier for the same purpose. In proof of this, Dr. Simpson adduced various extracts from the works of authors of the

Augustine age, as Dioscorides, Pliny, and Apuleius, &c.; as, for example, the following from Dioscorides' chapter on mandragore—

1. "Some boil down the roots in wine to a third part, and preserve the juice thus procured, and give one cyathus of it in sleeplessness, and severe pains of whatever part; also (it is given) *to cause the insensibility* (ποιεῖν ἀναίσθησιν) *of those who are to be cut or cauterized.*"

2. "There is prepared, also, besides the decoction, a wine from the bark of the root, three *minæ* being thrown into a cask of sweet wine; *and of this three cyathi are given to those who are to be cut or cauterized, as aforesaid; for, being thrown into a deep sleep, they do not perceive pain.*"

3. Again, of another mandragore called "*Morion*," Dioscorides observes, "medical men use it also for those who are to be cut or cauterized."

When treating of mandragore, Apuleius in the same way remarks, "If any one is to have a member mutilated, burned, or sawed [*mutilandum, comburendum, vel serrandum*], let him drink half an ounce with wine, and let him sleep till the member is cut away without any pain or sensation (*et tantum dormiet, quousque abscindatur membrum aliquo sine dolore et sensu*).

In describing the leaves of circeius or mandragore, Pliny remarks, "It has a soporific power on the faculties of those drinking it. An ordinary potion is half a cup. It is drunk against serpents, and before cuttings and puncturings, *lest they be felt.* (*Bibitur et contra serpentes, et ante sectiones punctionesque, ne sentiantur.*") The same author mentions that the seeds of rocket (*eruca*), when drunk infused in wine by criminals about to undergo the lash, produce a certain callousness or hardihood of feeling (*duritiam quandam contra sensum induere*). Dr. Simpson referred to Bang or Indian hemp being used in India with the same object at the present day; and cited various authorities to show that the gall and vinegar, or myrrhed wine offered to our Saviour before his crucifixion, was probably at that time generally employed with the same object. Dr. Simpson referred to the inhalation of the intoxicating vapours of hemp-seed, as having been practised by the Scythians long before the Christian era, as described by Herodotus, &c.

After some further similar observations, Dr. Simpson proceeded to draw the attention of the Society to the use of Chloroform, the powerful anæsthetic agent which he had lately discovered, in making a series of experiments on different respirable substances. The substance of these observations has been already given in this work.

CHAPTER II.

ACCOUNT OF A NEW ANÆSTHETIC AGENT, AS A SUBSTITUTE FOR SULPHURIC ETHER IN SURGERY AND MIDWIFERY.

"I esteem it, the office of a physician, not only to restore health, but to mitigate pain and
dolours."—BACON.

FROM the time at which I first saw ether-inhalation successfully practised in January last, I have had the conviction impressed upon my mind, that we would ultimately find that other therapeutic agents were capable of being introduced with equal rapidity and success into the system, through the same extensive and powerful channel of pulmonary absorption. In some observations, which I wrote and published in February last, relative to the inhalation of sulphuric ether in midwifery, I stated that, in several obstetric cases, I had used ergot of rye in this way, along with ether.—(See *Monthly Journal of Medical Science*, pp. 724 and 795, case of successful inhalation of opium, to arrest the vomiting of pregnancy.)

With various professional friends, more conversant with chemistry than I am, I have, since that time, taken opportunities of talking over the idea which I entertained of the probable existence or discovery of new therapeutic agents, capable of being introduced into the system by respiration, and the possibility of producing for inhalation vaporizable or volatile preparations of some of our more active and old-established medicines: and I have had, during the summer and autumn, ethereal tinctures, &c., of several potent drugs, manufactured for me, for experiment, by Messrs. Duncan, Flockhart, & Co., the excellent chemists and druggists of this city.

Latterly, in order to avoid, if possible, some of the inconveniences and objections pertaining to sulphuric ether,—(particularly its disagreeable and very persistent smell, its occasional tendency to irritation of the bronchi during its first inspirations, and the large quantity of it occasionally required to be used, more especially in protracted cases of labour,)—I have tried upon myself and others the inhalation of different other volatile fluids, with the hope that some one of them might be found to possess the advantages of ether, without its disadvantages. For

this purpose, I selected for experiment, and have inhaled several chemical liquids of a more fragrant or agreeable odour, such as the chloride of hydro-carbon (or Dutch liquid), acetone, nitrate of oxide of ethyle (nitric ether), benzin, the vapour of iodoform, &c.* I have found, however, one infinitely more efficacious than any of the others, viz., Chloroform, or the Perchloride of Formyle, and I am enabled to speak most confidently of its superior anæsthetic properties, having now tried it upon upwards of thirty individuals. The liquid I have used has been manufactured for me by Mr. Hunter, in the laboratory of Messrs. Duncan, Flockhart, & Co.

Chloroform was first discovered and described at nearly the same time by Soubeiran (1831), and Liebig, (1832); its composition was first accurately ascertained by the distinguished French chemist Dumas, in 1835.—See the *Annales de Chimie et de Physique*, vols. xlviii., xlix., and lviii. It has been used by some practitioners internally; Guillot prescribed it as an antispasmodic in asthma, exhibiting it in small doses, and diluted 100 times.—(See Bouchardat's *Annuaire de Therapeutique* for 1844, p. 35.) But no person, so far as I am aware, has used it by inhalation, or discovered its remarkable anæsthetic properties till the date of my own experiments.

It is a dense, limpid, colourless liquid, readily evaporating, and possessing an agreeable, fragrant, fruit-like odour, and a saccharine pleasant taste.

As an inhaled anæsthetic agent, it possesses over sulphuric ether the following advantages:—

1. A greatly less quantity of chloroform than of ether is requisite to produce the anæsthetic effect; usually from a hundred to a hundred and twenty drops of chloroform only being sufficient; and with some patients much less. I have seen a strong person rendered completely insensible by six or seven inspirations of thirty drops of the liquid.

2. Its action is much more rapid and complete, and generally more persistent. I have almost always seen from ten to twenty full inspirations suffice. Hence the time of the surgeon is saved; and that preliminary stage of excitement, which pertains to all narcotizing agents, being curtailed, or indeed prac-

* In talking over, with different chemists, what fluids might be sufficiently volatile to be respirable, and hence deserving of being experimented upon, Mr. Waldie first named to me the perchloride of formyle, as worthy, among others, of a trial;—Dr. Gregory suggested a trial of the chloride of hydrocarbon, &c. I have been deeply indebted to Dr. Gregory and Dr. Anderson, for their kindness in furnishing me with the requisite chemical agents for these experiments;—and also to my assistants, Dr. Keith and Dr. Dunean, for the great and hearty zeal with which they have constantly aided me in conducting the inquiry.

tically abolished, the patient has not the same degree of tendency to exhilaration and talking.*

3. Most of those who know from previous experience the sensations produced by ether inhalation, and who have subsequently breathed the chloroform, have strongly declared the inhalation and influence of chloroform to be far more agreeable and pleasant than those of ether.

4. I believe, that considering the small quantity requisite, as compared with ether, the use of chloroform will be less expensive than that of ether; more especially, as there is every prospect that the means of forming it may be simplified and cheapened.

5. Its perfume is not unpleasant, but the reverse; and the odour of it does not remain for any length of time, obstinately attached to the clothes of the attendant,—or exhaling in a disagreeable form from the lungs of the patient, as so generally happens with sulphuric ether.

6. Being required in much less quantity, it is much more portable and transmissible than sulphuric ether.

7. No special kind of inhaler or instrument is necessary for its exhibition. A little of the liquid diffused upon the interior of a hollow-shaped sponge, or a pocket-handkerchief, or a piece of linen or paper, and held over the mouth and nostrils, so as to

* In practice I have found that any such tendency, even with ether, is avoided by 1st, giving the patient from the first a large and overwhelming dose of the vapour, and 2dly, by keeping him perfectly quiet and still, and preventing all noise and talking around him. I have elsewhere insisted on the importance of these points. (See the numbers of the *Monthly Journal of Medical Science* for March, 1847, p. 726, and for September, p. 154.) In the paper last referred to, I took occasion, when discussing the conditions requisite for insuring successful etherization, to observe, “*First*, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherization, and during his recovery from it. All talking and all questioning should be strictly prohibited. In this way any tendency to excitement is eschewed, and the proper effect of the ether inhalation more speedily and certainly induced. And *Secondly*, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapour as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect; * * * a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotizing dose of the vapour. Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule;—not the principle of etherization, but the mode of putting it in practice being altogether to blame. But, *Thirdly*, whatever means or mode of etherization is adopted, the most important of the conditions required for procuring a satisfactory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife *until* the patient is under the full influence of the ether-vapour, and *thoroughly and indubitably soporized by it.*” In fulfilling all these indications, the employment of chloroform evidently offers great and decided advantages, in facility and efficiency, over the employment of ether.

be fully inhaled, generally suffices in about a minute or two to produce the desired effect.*

I have not yet had an opportunity of using chloroform in any capital surgical operation, but have exhibited it with perfect success, in tooth-drawing,† opening abscesses, for annulling the pain of dysmenorrhœa and of neuralgia, and in two or three cases where I was using deep, and otherwise very painful galvano-puncture for the treatment of ovarian dropsy, &c. I have employed it also in obstetric practice with entire success. The lady to whom it was first exhibited during parturition, had been previously delivered in the country by perforation of the head of the infant, after a labour of three days' duration. In this, her second confinement, pains supervened a fortnight before the full time. Three hours and a half after they commenced, and, ere the first stage of the labour was completed, I placed her under the influence of the chloroform, by moistening, with half a tea-spoonful of the liquid, a pocket handkerchief, rolled up into a funnel shape, and with the broad or open end of the funnel placed over her mouth and nostrils. In consequence of the evaporation of the fluid, it was once more renewed in about ten or twelve minutes. The child was expelled in about twenty-five minutes after the inhalation was begun. The mother subsequently remained longer soporose than commonly happens after ether. The squalling of the child did not, as usual, rouse her; and some minutes elapsed after the pla-

* When used for surgical purposes, perhaps it will be found to be most easily given upon a handkerchief, gathered up into a cup-like form in the hand of the exhibitor, and with the open end of the cup placed over the nose and mouth of the patient. For the first inspiration or two, it should be held at the distance of half an inch or so from the face, and then more and more closely applied to it. To insure a rapid and perfect anæsthetic effect—more especially where the operation is to be severe—one or two teaspoonfuls of the chloroform should be at once placed upon the hollow of the handkerchief, and immediately held to the face of the patient. Generally a snoring sleep speedily supervenes; and when it does so, it is a perfect test of the superinduction of complete insensibility. But a patient may be quite anæsthetic without this symptom supervening.

† A young dentist who has himself had two teeth extracted lately,—one under the influence of ether, and the other under the influence of chloroform,—writes me the following statement of the results:—"About six months ago I had an upper molar tooth extracted whilst under the influence of ether, by Mr. Imlach. The inhalation was continued for several minutes before I presented the usual appearance of complete etherization; the tooth was then extracted; and although I did not feel the least pain, yet I was conscious of the operation being performed, and was quite aware when the crash took place. Some days ago I required another molar extracted on account of toothache, and this operation was again performed by the same gentleman. I inhaled the vapour of chloroform, half a drachm being poured upon a handkerchief for that purpose, and held to my nose and mouth. Insensibility took place in a few seconds; but I was so completely *dead* this time, that I was not in the very slightest degree aware of anything that took place. The subsequent stupifying effects of the chloroform went off more rapidly than those of the ether; and I was perfectly well and able again for my work in a few minutes."

centa was expelled, and after the child was removed by the nurse into another room, before the patient awoke. She then turned round and observed to me that she had "enjoyed a very comfortable sleep, and indeed required it, as she was so tired,* but would now be more able for the work before her." I evaded entering into conversation with her, believing, as I have already stated, that the most complete possible quietude forms one of the principal secrets for the successful employment of either ether or chloroform. In a little time she again remarked that she was afraid her "sleep had stopped the pains." Shortly afterwards, her infant was brought in by the nurse from the adjoining room, and it was a matter of no small difficulty to convince the astonished mother that the labour was entirely over, and that the child presented to her was really her "own living baby."

Perhaps I may be excused from adding, that since publishing on the subject of Ether Inhalation in Midwifery, some time ago,† and then for the first time directing the attention of the medical profession to its great use and importance in natural and morbid parturition, I have employed it, with few and rare exceptions, in every case of labour that I have attended; and with the most delightful results. And I have no doubt whatever, that some years hence the practice will be general. Obstetricians may oppose it, but I believe our patients themselves will force the use of it upon the profession.‡ I have never had the pleasure of watching over a series of better and more rapid recoveries; nor once witnessed any disagreeable result follow to either mother or child; whilst I have now seen an immense amount of maternal pain and agony saved by its employment. And I most conscientiously believe that the proud mission of the physician is distinctly twofold—namely, to alleviate human suffering, as well as preserve human life.

CHEMICAL CONSTITUTION OF CHLOROFORM.

Formyle is the hypothetical radical of formic acid. In the red ant (*Formica rufa*) formic acid was first discovered, and hence its name. Gehlen pointed it out as a peculiar acid; and it was afterwards first artificially prepared by Doebereiner.

* In consequence of extreme anxiety at the unfortunate result of her previous confinement, she had slept little or none for one or two nights preceding the commencement of her present accouchement.

† See *Monthly Journal of Medical Science* for February, 1847, p. 639; for March, p. 718 and 721; and April, p. 794, &c.

‡ I am told that the London physicians, with two or three exceptions only, have never yet employed ether inhalation in their midwifery practice. Three weeks ago, I was informed in a letter from Professor Montgomery of Dublin, that he believed that in that city, up to that date, it had not been used in a single case of labour.

Chemists have now devised a variety of processes, by which formic acid may be obtained from starch, sugar, and, indeed, most other vegetable substances.

A series of chlorides of formyle are produced when chlorine and the hypochlorites are brought to act on the chloride, oxide, and hydrated oxide of methyle (pyroxylic or wood spirit). In the same way as formic acid may be artificially procured from substances which do not contain formyle ready formed,—so also are the chlorides of this radical capable of being procured from substances which do not originally contain it.

Chloroform, chloroformyle, or the perchloride of formyle, may be made and obtained artificially by various processes,—as by making milk of lime, or an aqueous solution of caustic alkali act upon chloral,—by distilling alcohol, pyroxylic spirit, or acetone, with chloride of lime,—by leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c. The preparation which I have employed, was made according to the following formula of Dumas:—

" R Chloride of lime in powder,	℔. IV.
Water,	.	:	:	:	℔. XII.
Rectified Spirit,	f 3 XII.

" Mix in a capacious retort or still, and distil as long as a dense liquid, which sinks in the water with which it comes over, is produced."—(Gray's *Supplement to the Pharmacopœia*, 1846, p. 633.)

The resulting perchloride of formyle consists of two atoms of carbon, one of hydrogen, and three of chlorine. Its specific gravity is much greater than that of water, being as high as 1.480. It boils at 141°. The density of its vapour is 4.2. It is not inflammable; nor changed by distillation with potassium, potash, sulphuric, or other acids.—(See Turner's *Elements of Chemistry*, 8th edition, p. 1009; Gregory's *Outlines of Chemistry*, part ii. p. 401; Fownes' *Manual of Elementary Chemistry*, p. 419; Thomson's *Chemistry of Organic Bodies*, p. 312; Loewig's *Organische Chemie*, vol. i. p. 498.)

It is now well ascertained that three compound chemical bodies possess, when inhaled into the lungs, the power of super-inducing a state of anæsthesia, or insensibility to pain in surgical operations, &c., namely, nitrous oxide, sulphuric ether, and perchloride of formyle. The following tabular view shows that these agents are entirely different from each other in their chemical constitution, and hence that their elementary composition affords no apparent clue to the explanation of their anæsthetic properties:—

	Propor. of Nitrogen.	Propor. of Oxygen.	Propor. of Carbon.	Propor. of Hydrogen.	Propor. of Chlorine.
Nitrous Oxide,	1 Atom.	1 Atom.	—	—	—
Sulphuric Ether,	—	1 Atom.	4 Atoms.	5 Atoms.	—
Chloroform,	—	—	2 Atoms.	1 Atom.	3 Atoms.

It is perhaps not unworthy of remark, that when Soubeiran, Liebig, and Dumas engaged, a few years back, in those inquiries and experiments by which the formation and composition of chloroform was first discovered, their sole and only object was the investigation of a point in philosophical chemistry. They laboured for the pure love and extension of knowledge. They had no idea that the substance to which they called the attention of their chemical brethren could or would be turned to any *practical* purpose, or that it possessed any physiological or therapeutic effects upon the animal economy. I mention this to show, that the *cui bono* argument against philosophical investigations, on the ground that there may be at first no apparent practical benefit to be derived from them, has been amply refuted in this, as it has been in many other instances. For I feel assured, that the use of chloroform will soon entirely supersede the use of ether; and, from the facility and rapidity of its exhibition, it will be employed as an anæsthetic agent in many cases, and under many circumstances, in which ether would never have been had recourse to. Here then we have a substance which, in the first instance, was merely interesting as a matter of scientific curiosity and research, becoming rapidly an object of intense importance, as an agent by which human suffering and agony may be annulled and abolished, under some of the most trying circumstances in which human nature is ever placed.

POSTSCRIPT.—Since the above observations were sent to the press, I have—through the great kindness of Professor Miller and Dr. Duncan—had an opportunity of trying the effects of the inhalation of chloroform, to-day, in three cases of operation in the Royal Infirmary of Edinburgh. A great collection of professional gentlemen and students witnessed the results, and among the number was Professor Dumas of Paris, the chemist who first ascertained and established the chemical composition of chloroform. He happened to be passing through Edinburgh, engaged along with Dr. Milne Edwards, who accompanied him, in an official investigation for the French government,—

and was, in no small degree, rejoiced to witness the wonderful physiological effects of a substance with whose chemical history his own name was so intimately connected.

I append notes obligingly furnished to me by Professor Miller and Dr. Duncan, of the three cases of operation. The first two cases were operated on by Professor Miller; the third by Dr. Duncan. In applying the chloroform in the first case, I used a pocket-handkerchief as the inhaling instrument; in the last two I employed a hollow sponge.

CASE I.—“A boy, four or five years old, with necrosis of one of the bones of the fore-arm. Could speak nothing but Gaelic. No means, consequently, of explaining to him what he was required to do. On holding a handkerchief, on which some chloroform had been sprinkled, to his face, he became frightened, and wrestled to be away. He was held gently, however, by Dr. Simpson, and obliged to inhale. After a few inspirations he ceased to cry or move, and fell into a sound snoring sleep. A deep incision was now made down to the diseased bone, and, by the use of the forceps, nearly the whole of the radius, in the state of sequestrum, was extracted. During this operation, and the subsequent examination of the wound by the finger, not the slightest evidence of the suffering of pain was given. He still slept on soundly, and was carried back to his ward in that state. Half an hour afterwards, he was found in bed, like a child newly awakened from a refreshing sleep, with a clear merry eye, and placid expression of countenance, wholly unlike what is found to obtain after ordinary etherization. On being questioned by a Gaelic interpreter, who was found among the students, he stated that he had never felt any pain, and that he felt none now. On being shown his wounded arm, he looked much surprised, but neither cried nor expressed the slightest alarm.”

CASE II.—“A soldier who had an opening in the cheek—the result of exfoliation of the jaw—was next made to inhale. At first he showed some signs of moving his hands too freely; but soon also fell into a state of sleep and snoring. A free incision was made across the lower jaw, and from this the dense adhering integuments were freely dissected all round, so as to raise the soft parts of the cheek. The edges of the opening were then made raw, and the whole line of incision was brought together by several points of suture. This patient had previously undergone two minor operations of a somewhat similar kind; both of them had proved unsuccessful, and he bore them very ill—proving unusually unsteady, and complaining bitterly of severe pain. On the present occasion, he did not wince or moan in the slightest degree; and, on the return of conscious-

ness, said that he had felt nothing. His first act, when apparently about half awake, was suddenly to clutch up the sponge with which the chloroform was used, and readjust it to his mouth, obviously implying that he had found the inhalation from it anything but a disagreeable duty.

"This case was further interesting as being one of those operations in the region of the mouth, in which it has been deemed impossible to use ether,—and certainly it would have been impossible to have performed the operation with any complicated inhaling apparatus applied to the mouth of the patient."

CASE III.—"A young man, of about twenty-two years of age, having necrosis of the first phalanx of the great toe, and ulceration of the integuments, the consequence of injury. The ulcerated surface was exceedingly tender to the touch—so much so, that he winced whenever the finger was brought near to it; and the slightest pressure made him cry out. After the removal of the dressings, which caused some pain and fretting, the inhalation was commenced, and the patient almost immediately* became insensible, and lay perfectly still, while the diseased mass was being removed by amputation of the toe through the middle of the second phalanx. The inhalation was now stopped. The edges of the wound were then brought together with three stitches, and the wound dressed. The patient shortly afterwards awoke, looked round him, and gratefully declared his entire and perfect freedom from all pain and uneasiness during the operation."

The whole quantity of chloroform used in these three operations did not exceed half an ounce,—and, as Professor Miller afterwards observed to the students that were present, if ether had been used, several ounces of it would have been requisite to produce the same amount of anæsthetic effect.

The following case occurred also to-day, to Mr. Miller, in private practice. The notes of it and the subsequent remark are in his own words.

CASE IV.—"A young lady wished to have a tumour (en-cysted) dissected out from beneath the angle of the jaw. The chloroform was used in small quantity (about a drachm), sprinkled upon a piece of operation sponge. In considerably less than a minute she was sound asleep, sitting easily in a chair, with her eyes shut, and with her ordinary expression of countenance. The tumour was extirpated, and a stitch inserted, without any pain having been either shown or felt. Her sensations, throughout, as she subsequently stated, had been of

* Dr. Christison, who was watching the result, informs me that this patient was affected in half a minute.

the most pleasing nature ; and her manageableness during the operation was as perfect as if she had been a wax doll or a lay figure.

"No sickness, vomiting, headache, salivation, uneasiness of chest, in any of the cases. Once or twice a tickling cough took place in the first breathings."

I have, up to this date, exhibited the chloroform to about fifty individuals. In not a single instance has the slightest bad result of any kind whatever occurred from its employment.

EDINBURGH, *15th November, 1847.*

CHAPTER III.

ANÆSTHETIC AND OTHER THERAPEUTIC PROPERTIES OF CHLOROFORM.

AT the first winter meeting of the Edinburgh Medico-Chirurgical Society (10th November), I directed the attention of the members to a new respirable anæsthetic agent which I had discovered a short time previously,—viz., Chloroform, Chloroformyle, or Perchloride of Formyle. In this limited notice, I shall state briefly some of the principal facts pertaining to its history, composition effects, &c.

Chemical History and Composition.—Chloroform was discovered at nearly the same time by Soubeiran (1831), and Liebig (1832). Its chemical composition was first ascertained by Dumas and Peligot (1835). It consists of 2 atoms of carbon, 1 of hydrogen, and 3 of chlorine; or, to express it otherwise, of 1 atom of formyle, and 3 of chlorine. Hence its chemical formula is $C_2 H Cl_3$; or $Fo Cl_3$.

Modes of Preparation.—It may be obtained by various processes. 1. By the distillation of a mixture of diluted spirit, pyroxylic or wood spirit, or acetone, and chloride of lime (bleaching powder); or, 2. By making milk of lime, or an aqueous solution of caustic alkali, act upon chloral; 3. By leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c.

Physical and Chemical Properties.—It is a clear limpid liquid, as heavy as 1.480; not inflammable; very volatile; and boils at 141° . It has a fragrant, fruitlike odour; and a sweet saccharine taste.

Therapeutic History.—It has been used internally. Guillot employed it in asthma, diluted with water one hundred times (1844). My friend, Dr. Formby of Liverpool, told me, about two years ago, that he used it often in a diluted form as a diffusible stimulant; and I have, since that period, frequently prescribed it instead of valerian, camphor, &c.* But I am not aware that any person has used chloroform by inhalation, or

* Since first publishing on the subject of chloroform, Dr. Glover of Newcastle has pointed out to me, that, in an essay on Bromine in the 152d number of the Edinburgh Medical and Surgical Journal, he mentions having poisoned several animals with chloroform, by injecting it into their blood-vessels, stomach, and the cavity of the peritoneum, and has investigated its physiological mode of action.

discovered its remarkable anæsthetic properties till the date of my own experiments.

Advantages as an Anæsthetic Agent.—In producing insensibility to pain in surgical and obstetric practice, chloroform possesses various important advantages over sulphuric ether. 1. A greatly less quantity of chloroform is required; 2. Its action is much more rapid, more perfect, and generally more persistent; 3. Its exciting or exhilarating stage is far shorter, insensibility commonly supervening in a minute or two, or less; hence, 4. The time of the surgeon is saved; 5. The inhalation and influence of it are more agreeable and pleasant; 6. Its odour is evanescent; 7. No special instrument is required for its employment.

Dose and Mode of Exhibition.—A fluid drachm or two of the liquid, diffused upon the interior of a pocket-handkerchief, arranged in a concave or cuplike form in the hand of the exhibitor, and applied over the nose and mouth of the patient, generally suffices to produce rapid and complete anæsthesia. A few patients may require more, others less. The *edges* of the cup or cone are not to be wetted, or the patient's face will be irritated. To keep up its action, when that is necessary, the handkerchief must be again besprinkled with the fluid when the first quantity is evaporated. The moistened handkerchief should be at first held at the distance of about half an inch from the face, and gradually approached nearer. The patient should, if possible, be placed easily and upon his back, and advised previously to take full inspirations. All noise and excitement around the patient should be strictly and peremptorily forbidden.*

Physiological Effects.—After the first two or three full inspirations, a feeling of warmth and excitation, radiating from the chest to the extremities; followed by whirring noises in the ears; a sensation of vibratory thrilling and benumbing throughout the body; with, betimes, rapid loss of sensation and of motion, and at last of consciousness. Often before total unconsciousness supervenes, the patient, guided by instinct rather than by volition and reason, makes an effort to get rid of the inhaling vapour and handkerchief, as if it interfered with free respiration. This temporary effort must be resisted by the exhibitor. During the full anæsthetic sleep produced by chloroform, sometimes no mental action goes on, or at least is remembered; in many others, the mind is active as in dreams.

* Nothing can be more absurd than the way in which some dentists proceed. The muscles of the jaws often close under the use of chloroform and ether; and they try to open the mouth partly by persuasion, partly by force—irritating and rousing the patient. A cork or gag placed between the teeth, *before* the inhalation is commenced, saves all this, and expedites and facilitates the whole process.

The respiration is usually at first soporose; the pupil sometimes natural, in others slightly contracted, in others dilated; the pulse is usually quickened ten or twenty beats at first, but afterwards falls to its normal rate, and, if the vapour is exhibited very long in very powerful doses, it comes down more and more below the natural standard; muscles of voluntary motion in general relaxed; more rarely cataleptic; still more rarely clonically contracted, as happens also occasionally with ether.

In small doses, given slowly, its effects are exhilarating; and exactly like those generally following the inhalation of nitrous oxide gas. Of course, when exhibited in this way, the patient is in a state of excitement quite unfit for a surgical operation. When given for surgical operations, it should be exhibited rapidly in large doses, and the patient sent over into a deep soporose or stertorous sleep *before* the incisions are begun.*

Uses in Surgery.—1. To relax the muscles in reducing dislocations, &c.; 2. To avert the sufferings attendant on deep probing, and other painful but necessary modes of diagnostic examination and dressing; and 3d, and principally, to annul the pain of operations by the caustic, ligature, or knife.

Examples.—I. A child of ten weeks old had a very large nævus behind the ear. Dr. Duncan destroyed its internal organization by passing large red-hot needles in different directions through it. While the tumour was hissing and decomposing under their action, the infant lay quietly and placidly asleep on my knee, under the influence of chloroform. This is the youngest subject to whom I have given it.—II. A boy, of four or five, had a necrosed radius cut down upon, and removed by Mr. Miller. He slept soundly during the operation; and, without moving, he was carried out of the operation theatre of the hospital still fast asleep. When visited some time afterwards, he was found awake in bed, with a bright merry eye, as if just out of a refreshing sleep. No pain even then.—III. A nervous woman, a patient of Mr. Miller's, was to undergo partial amputation of the foot in the hospital—afraid both of the operation, and of being carried in before a crowd of medical men for the purpose. I apathized her with chloroform in the consulting-room of the hospital, had her carried into the operation-room in that state, and did not allow her to awake till the amputation was performed, and she was removed back again to bed. She was thus entirely spared both the moral shock and physical pain which she dreaded.—IV. A boy had his elbow-joint excised by

* I believe all the reputed failures and misadventures are attributable to two causes, viz., 1, using an impure and imperfect variety of chloroform; and 2, not giving it in sufficiently large and rapid doses.

Mr. Syme. The operation, which is always a very painful one, was prolonged in consequence of the very diseased state of the parts operated on. He slept soundly, and remained perfectly and passively still during the whole operation. &c. &c.

Uses in Midwifery.—To diminish and annul the physical pains attendant on labour, and more especially those which accompany the passage of the child's head through the pelvic cavity and outlet, (the second stage of Denman.)

Examples.—I. The lady to whom it was first exhibited had been previously delivered in the country by craniotomy after a very long labour. Her second confinement took place a fortnight before the full time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded, and the pains very severe. In twenty-five minutes the child was born. The mother did not awake till after the placenta was removed; and was perfectly unaware that her child was born and alive. She stated her sensations to be those of awaking from "a very comfortable sleep."—II. I exhibited it, with Mr. Carmichael, to a patient who had, at her preceding confinement, been in severe labour for twenty hours—followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body, and bed-clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.—III. Patient unmarried. A first labour. Twins. The first child presented by the pelvis, the second with the hand and head. The chloroform was exhibited when the os uteri was fully dilated. The passages speedily became greatly relaxed (as has happened in other cases placed under its influence), and in a few pains the first child was born, assisted by some traction. I broke the membranes of the second, pushed up the hand, and secured the more complete presentation of the head. Three pains expelled the child. The mother was then bound up; her clothes were changed; and she was lifted into another bed. During all this time she slept on soundly, and for a full hour afterwards; the chloroform acting in this, as in other cases of its prolonged employment, as a soporific. The patient recollected nothing from the time of the first inhalations, and was greatly distressed when, not one, but two living children were brought by the nurse to her. Dr. Christison saw this case with me. I have used it in several operative deliveries with similar success.

In labour it does not require to be given in such large doses

as in surgery. After the first *full* dose, a few inhalations before each returning uterine contraction is generally sufficient. It should be made more deep as the head is passing the perineum and vulva. If the state is extremely and unnecessarily deep, it will no doubt diminish and even temporarily stop uterine contractions; and I have taken advantage of this, in one case, in facilitating the operation of turning, &c. Besides thus, 1. Diminishing or annulling the more severe part of the sufferings attendant on natural labour, it will, 2. Abolish those more agonizing pains which accompany the use of the forceps, and other modes of operative delivery; 3. Enable us to extract the placenta artificially when required, without resistance or suffering; and 4. Give us the power of making an accurate and full examination of the presentation, when necessary early in labour, as in placenta previa, preternatural presentations, &c.

Uses in Medicine.—1. As an *antispasmodic*; as in asthma, laryngismus, tetanus, and other spasmodic diseases, &c.* I have used successfully the inhalation of ether to arrest the paroxysms of hooping-cough, dysmenorrhœa, colic, and the pains attendant on the passage of biliary calculi. In a case of the most severe, at the same time painful, spasmodic twisting and convulsions of the extremities attending a second attack of chorea, I allowed the patient ether-inhalation; and sometimes she lay under its influence for hours, with relief while its action lasted, but generally without sleep. Latterly the chloroform has both relieved spasms and their attendant pain, and procured sleep.—2. As an

* In various trials at Morningside, at which Dr. Christison, Dr. Skæ, and Dr. Wingett were present, Dr. S. had set violent patients over into a soporose sleep in a minute or less. As to what its therapeutic effects, if any, might be in insanity, we had, he believed, no power yet of judging. It could not be expected to be of any marked service—at least in any short time—in such chronic cases, as it had been tried in. But it had this effect: you could keep the patients asleep under it for a long series of hours. In this way, it had already apparently cured some cases of delirium tremens, and, he believed, also of puerperal insanity, and might yet be found useful in other forms of acute mania. At all events, it was a means of restraining a furious maniac, as powerful, and, perhaps, it would be found far more safe than a strait-jacket or the grasp of a number of keepers. Once set a patient over (and that was the work of a minute), and a nurse with a pocket-handkerchief and some chloroform might keep him under perfect and complete restraint. Nor need he add how useful the same means might be in enabling a riotous and resisting patient to be removed to an asylum, or from one place to another. It would be tedious to discuss all the other diseases in which it had already been tried. Dr. S. was most anxious to observe its effects in acute local inflammations, but he had, as yet, few opportunities of doing so. He mentioned a case he had seen with Dr. Dease (apparently an extra-uterine conception bursting into the abdomen), and where the accompanying severe abdominal pain, and its dreadfully depressing effects, were kept at bay by the anæsthetic and contro-stimulant effects of the chloroform. He mentioned a case of extremely severe *cholera*, where, after all things had failed, and the patient was apparently sinking, the inhalation of chloroform had induced sleep,—dispelled the spasms and vomiting—and restored the patient. (*From the Discussion at the Medico-chirurgical Society of Edinburgh, on December 15, 1847. See Monthly Journal of Medical Science for January, 1848.*)

anodyne or *narcotic*. In neuralgia, I have seen chloroform stop the fit at once; in two other cases the pain remained absent only while the chloroform acted. A patient suffering under severe delirium tremens had remained awake for about seventy hours; a half ounce of laudanum, given at a single dose, failed to produce rest; ten hours afterwards, the inhalation of chloroform was immediately followed by several hours of critical sleep. What cases of insanity would it benefit? I have exhibited it in full doses in some cases of dementia, combined with excitement and wakefulness. They were all asleep in about a minute—and remained so for some time. In nothing does chloroform differ from ether more than in its soporific effects—when given in full doses, and continued for some time.—3. In small doses as a *diffusible stimulant*; to arrest the first commencement of ague, ephemera, &c.; in hysteria, &c. Perhaps it may be used by inhalation in small quantities when the stomach will not bear wine or other stimulants; in severe vomiting, fevers, &c. I have seen its inhalation at once dispel a sick headache.—4. As a *contro-stimulant* in inflammatory diseases, especially those of a painful character?? Acute rheumatism, &c.??

Cautions.—The liquid used should be sufficiently strong. Its proper sp. gr. is (as I have said) 1.480. I have seen some specimens perfectly unfit for use from their weakness; others perfectly unfit from their want of purity, for although of the proper specific gravity, they contained free chlorine, irritating the throat, and making the patient cough. It is certainly far too powerful an agent to be intrusted to nurses or unprofessional individuals. I have given it, up to this date, to above eighty persons, without the slightest bad result of any kind whatever in any one of them. The power, however, which we have with it, of bringing down the pulse, &c., shows that, if exhibited in *too strong* a dose, given *uninterruptedly* for too great a length of time, it would doubtless produce serious consequences, and even death. But, certainly, all its *full* anæsthetic and other influences may be perfectly obtained without allowing it to produce such depression as would be in any degree dangerous. I have, in obstetric cases, kept patients under its influence for several consecutive hours, without any resulting harm of any kind to either mother or child; renewing its application from time to time after the first full dose. Like many other agents, it may be powerful for evil as well as for good. I believe its great potency will be one great safeguard against its abuse.

Its influence upon the blood, &c., the counter-indications to its use, &c. &c., remain still to be ascertained.

CHAPTER IV.

NOTES ON THE ANÆSTHETIC EFFECTS OF CHLORIDE OF HYDROCARBON,
NITRATE OF ETHYLE, BENZIN, ALDEHYDE, AND BISULPHURET OF
CARBON.

DURING the last few months two or three different substances have been brought forward as anæsthetic agents; but our medical journals have afforded little or no detailed notice of their effects. The few following notes, however imperfect, may not therefore be uninteresting; more particularly as they are the result of direct experiments upon myself and others with the agents in question. In most of these experiments, I had the kind and able assistance of Dr. Keith and Dr. Duncan.

When first publishing, in November, 1847, upon the anæsthetic properties of chloroform, I stated that, "in making a variety of experiments upon the inhalation of different volatile chemical liquids, I have, in addition to perchloride of formyle, breathed chloride of hydrocarbon, acetone, nitrate of oxide of ethyle, benzin, the vapour of iodoform, &c. I may probably (I added) take another opportunity of describing the result."—(See *Lancet* for 20th November, 1847, p. 549.)

Three of the substances which I named in the preceding list, produce, when inhaled, a state of anæsthetic insensibility:—viz., chloride of hydrocarbon, nitrate of oxide of ethyle, and benzin.

CHLORIDE OF HYDROCARBON.

Chloride of Hydrocarbon, or *Dutch liquid*, as it is often termed in consequence of it being first discovered by the Dutch chemists of the last century, is one of the various fluids to which the name of *Chloric Ether* was for some time given.

When equal parts of olefant gas and chlorine are mixed together, the two gases rapidly disappear, and produce a colourless oily liquid, of a peculiar sweetish taste and ethereal odour. Its specific gravity is 1.247. It boils at 148°. It is composed of four atoms of carbon, four of hydrogen, and two of chlorine. Hence its formula is $C_4 H_4 Cl_2$.

When its vapour is inhaled, the chloride of hydrocarbon causes so great irritation of the throat, that few can persevere in breathing it for such a length of time as to induce anæsthesia.

I have latterly, however, seen it inhaled perseveringly until this state, with all its usual phenomena, followed; and without excitement of the pulse, or subsequent headache. When I myself attempted to inhale the chloride of hydrocarbon, it produced an extreme degree of acrid irritation in the throat, which did not disappear entirely for many hours afterwards.

NITRATE OF ETHYLE.

When two parts of alcohol, and one part of pure nitric acid, are distilled together, with the addition of a small quantity of urea, *Nitrate of Ethyle*, or, more properly, *Nitrate of Oxide of Ethyle*, is produced. It is a transparent colourless liquid, with a sweet taste, and very agreeable odour. Its specific gravity is 1.112; it boils at 185°. It is a compound of four proportions of carbon, five of hydrogen, six of oxygen, and one of nitrogen; and its formula is, $(C_4 H_5) O, NO_5$; or $Ac O, NO_5$.

Nitrate of ethyle is easy and pleasant to inhale, and possesses very rapid and powerful anæsthetic properties. A small quantity, such as fifty or sixty drops, when sprinkled on a handkerchief and inhaled, produces insensibility after a few inspirations. But during the brief period which elapses before the state of complete anæsthesia is induced, the sensations of noise and fullness in the head are in general excessive; and much headache and giddiness have usually followed its employment, and persisted for some time.

BENZIN.

Benzin or *Benzole* was first discovered by Faraday, as one of the products in his experiments on compressing oil gas, and was designated by him *Bicarburet of Hydrogen*. Mitscherlich afterwards obtained it by distilling, at a high temperature, benzoic acid with an excess of slaked lime.

It is a clear colourless liquid, of a peculiar ethereal odour; with a specific gravity of 0.85; and boils at 186°. It is believed to be composed of two proportions of carbon and one of hydrogen. Its formula is, $C_8 H_6$; or perhaps, more properly, $C_{12} H_6$. It is polymeric with the hypothetical radical formyle.

In my experiments with benzin I found it capable of producing anæsthesia; but the ringing and noises in the head accompanying and following its inhalation, were so excessive, and almost intolerable in the case of myself and others, as to seem to us to render its practical applications impossible, even had there been no other objections to its use. Latterly, Dr. Snow has tried its employment upon some patients for tooth-drawing; and in one instance of amputation. In this last case it produced convulsive tremors.—(*Lancet* for 12th February, 1848, p. 180.)

ALDEHYDE.

Aldehyde, or Hydrate of Oxide of Acetyle, was first noticed by Dæbereiner in distilling together sulphuric acid, alcohol, and peroxide of manganese; but it was left for Liebig to fix and determine everything about its chemical nature. It is a colourless limpid liquid, of specific gravity 0.791. It is very volatile, boiling at 72° . It spontaneously changes when long kept, and is converted into two substances, a solid and a fluid, metaldehyde and elaldehyde. Liebig found it to be composed of four atoms of carbon, four atoms of hydrogen, and two of oxygen; and its formula is $C_4H_4O + aq.$

Professor Poggiale of Paris, has lately made some experiments with dogs on the inhalation of the vapour of aldehyde, and from these has concluded that its anæsthetic effects will be found more prompt and energetic than those of sulphuric ether or chloroform. It certainly possesses, like some of the preceding agents, well-marked anæsthetic properties; but it assuredly will never come into use, as very few will be found capable of inhaling a sufficient dose of its vapour. In fact, out of five of us that attempted to inhale aldehyde, very carefully prepared and purified, four were driven to suspend the respiration of it in consequence of the coughing and insufferable feelings of dyspnœa which it immediately induced. The sensations of difficult respiration and constriction in the chest which the vapour produced, resembled precisely those of a severe fit of spasmodic asthma. In the fifth case, the experimentalist, after perseveringly breathing the aldehyde for a minute or two, became entirely insensible; the state of anæsthesia lasted for two or three minutes; during it, the pulse became excessively small and feeble. On recovering, the bronchial constriction and coughing, which had disappeared as the anæsthesia was induced, returned immediately, and was annoying for some time.

BISULPHURET OF CARBON.

Bisulphuret of Carbon, or Alcohol of Sulphur (as it was at first termed), was accidentally discovered in 1796 by Lampadius, when experimenting on iron pyrites. Different opinions of its composition were held by different chemists; but Berzelius and Marcet, in 1813, at last fully confirmed the previous idea of Clement and Desormes, Vauquelin, &c., that it consisted only of sulphur and carbon. It is composed of two atoms of the former to one of the latter; consequently its formula is CS_2 .

The most easy method of procuring it is by transmitting the vapour of sulphur over fragments of charcoal heated to redness in a closed porcelain or iron tube. The resulting bisulphuret of carbon, when purified by distillation, is a clear, colourless liquid, of a pungent taste. Its specific gravity is 1.272. It is very volatile, boiling at 108°.

It has been stated in various literary journals, that bisulphuret of carbon has lately been used as an anæsthetic agent at Christiana; but no particulars regarding its employment in Norway have, as far as I know, been yet published.

I have breathed the vapour of bisulphuret of carbon, and exhibited it to about twenty other individuals, and it is certainly a very rapid and powerful anæsthetic. One or two stated that they found it even more pleasant than chloroform; but in several it produced depressing and disagreeable visions, and was followed for some hours by headache and giddiness, even when given only in small doses. In one instance I exhibited it, with Mr. Miller's permission, to a patient, from whom he removed a tumour of the mamma. It very speedily produced a full anæsthetic effect; but it was difficult to regulate it during the operation. The patient was restless in the latter part of it; but felt nothing. Like several others when under it, her eyes remained wide open. After the operation she was extremely sick, with much and long-continued headache; and, for fifty or sixty hours subsequently, her pulse was high and rapid, without rigor or symptoms of fever.

I tried its effects in a case of midwifery, in presence of Dr. Wier, Dr. Duncan, Mr. Norris, and a number of the pupils of the Maternity Hospital. It was employed at intervals during three quarters of an hour. The patient was easily brought under its influence, a few inspirations sufficing for that purpose; but it was found altogether impossible to produce by it the kind of continuous sleep attending the use of chloroform. Its action was so strong, that when given, as a pain threatened or commenced, it immediately affected the power of the uterine contractions, so as often to suspend them; and yet its effects were so transient that the state of anæsthesia had generally passed off within a minute or two afterwards. The patient anxiously asked for it at the commencement of each pain. During its use she was occasionally sick, and vomited several times. Latterly her respiration became rapid, and her pulse rose extremely high. I then changed the inhalation for chloroform, and, under it, the patient slept quietly on for twenty minutes, when the child was born. During these twenty minutes there was no more sickness or vomiting, and the pulse gradually sunk down to its natural standard. A few minutes after the

child was expelled, and while the mother still slept, her pulse was counted at 80. Next day the mother and infant were both well, and she has made a good recovery.

While these experiments prove the strong anæsthetic properties of bisulphuret of carbon, they at the same time show its disadvantages. I have not alluded to another strong drawback upon its use, viz., its very unpleasant odour. "It has (says Dr. Gregory) a peculiarly offensive smell of putrid cabbage."—(*Outlines of Chemistry*, p. 130.) By dissolving various essential oils in the bisulphuret I tried to overcome this disagreeable defect, but without much success.

None of the five anæsthetics which I have mentioned in the present communication are, I believe, comparable with chloroform or sulphuric ether, either in their manageableness or in their effects. And the after-consequences which all of them tend to leave, are too severe and too frequent to admit of their introduction into practice. They are more interesting physiologically than therapeutically.

EDINBURGH, *March 20, 1848.*

PART IV.

ON LOCAL ANÆSTHESIA.

CHAPTER I.

HISTORICAL NOTICE ON THE PRODUCTION OF ARTIFICIAL LOCAL ANÆSTHESIA.

A FEW months ago I published some remarks with the object of showing that the artificial production of a state of general anæsthesia before the performance of surgical operations was not altogether an idea of modern times. Several ancient writers aver that such a state of general anæsthesia can be produced by using mandragore,—as Dioscorides, Pliny, Apuleius, &c. In the thirteenth century, Theodoric published a receipt for producing it by the *inhalation* of vapours arising from the watery extracts of various narcotic herbs; and in our own days (1832), a receipt similar to this of Theodoric's has been, apparently, found quite sufficient for the purpose by Dauriol.

The ancients, also, seem to have entertained the idea of the possibility of producing a state of *local* and limited anæsthesia in any part to be operated upon. Dioscorides, who, as we have seen, repeatedly mentions the production of *general* anæsthesia by mandragore, states it as a matter of report that *local* anæsthesia in a part was capable of being produced by the Memphian stone. "The Memphian stone (says he) is found in Egypt near Memphis, of the size of a calculus, fatty and of different colours. They say that this, when bruised, and spread over parts that are to be cut or cauterized, produces in them a dangerless anæsthesia."

Whilst we may entirely doubt whether local anæsthesia was capable of being produced by such an apocryphal application as the above, the passage is curious, as evidence that the idea of

obtunding a single part of the body against the pain of an operation, was not unknown or unthought of in former times. Nay, many old authorities believed that against the fire-ordeal, any part of the body could be so protected and defended by previous applications, that the human hand, for instance, should not feel the contact of red-hot iron. The writings of Eusebe Salverte, and of Beckmann, contain ample notices on this disputed question. Upwards of half a century ago, our countryman, Dr. Moore, ingeniously proposed to effect a local anæsthesia of any limb that was to be operated upon by previously compressing with tourniquets and pads the nervous trunks going to that limb; and he has left us an interesting account*

* See his "*Method of Preventing or Diminishing Pain in several Operations of Surgery*," London, 1784.—"I communicated (says Mr. Moore, p. 30), the experiments I had made, and all my ideas on the subject to Mr. Hunter, who was so obliging as immediately to offer me an opportunity of trying the effect of my compressor, at St. George's Hospital, on a man whose leg he was to take off below the knee within a few days. I went to the Hospital the day before the operation to try the instrument. The patient had lost all his toes, and had a large ulcer on his foot. This was so much inflamed and so irritable that dressing it in the gentlest manner, gave him the most acute pain. I applied the instrument; after the compression had been continued for about half an hour, his limb became so insensible that rubbing pretty smartly with the finger upon the ulcer gave no pain. Next morning, the patient being carried into the operation room, I began the compression of the nerves at a quarter before eleven o'clock. The numbness of the limbs followed at the usual time. At a quarter before twelve I gave him one grain of opium to diminish the smarting of the wound after the operation, when the compression should be taken off. A few minutes after twelve the tourniquet was applied, and the amputation performed by Mr. Hunter, at the usual place below the knee.

"At the circular incision through the skin, the patient did not cry out, change a muscle of his face, or show any symptoms of pain. At the subsequent parts of the operation, particularly during the sawing of the bones, he showed marks of uneasiness in his countenance, but did not cry out. As it was thought necessary to take up no less than five arteries, the operation lasted a longer time than is usual, and towards the end he grew faintish, and desired to have some water, and afterwards asked if they were nearly done. When the operation seemed to be over, and the bleeding stopt, the tourniquet was relaxed, and I also removed the compressor; but a small vessel bleeding unexpectedly, it was thought necessary to tie it also. Here the patient showed very strong marks of pain, and afterwards declared that tying this last vessel gave him much more pain than all the others, although the great nerves had been included in the ligatures. When he was put to bed the wound smarted, as is usual after amputations. The compressor being now entirely removed, this was to be expected. But some time after, being questioned concerning the pain he had suffered during the operation, he declared that he had felt hardly any, except (as he himself expressed it), at the rasping of the bones, which, he added, had shaken his whole limb. This seems a little extraordinary, as sawing the bones is usually the least painful part of amputations. * * *

"This trial had all the success I expected; there was evidently a most remarkable diminution of pain, particularly during the first incisions through the skin and muscles, which are generally by far the most severe parts of the operation, and I am convinced that what pain the patient felt was chiefly owing to some small branches of the lumbar nerves which extend below the knee, and were not compressed." Perhaps some of our modern surgeons will not consider inclusion of the great nerves in the arterial ligatures as any compliment to Mr. Hunter's surgery.

of a case of amputation at St. George's Hospital, in which this plan was tried, apparently with partial success, by John Hunter.

The possible production of local anæsthesia by this or other means is certainly an object well worthy of study and attainment. Surgeons everywhere seem to be more and more acknowledging the facility, certainty, and safety with which the state of general anæsthesia can be produced at will before operating, and the moral and professional necessity of saving their patients from all unrequisite pain. But if we could by any means induce a local anæsthesia without that temporary absence of consciousness which is found in the state of general anæsthesia, many would regard it as a still greater improvement in this branch of practice. If a man, for instance, could have his hand so obtunded that he could *see* and yet not *feel* the performance of amputation upon his own fingers, the practice of anæsthesia in surgery would, in all likelihood, advance and progress still more rapidly than ever it has done.

In the following remarks it is my object to state the results of a number of experiments which I have performed,—1st, upon the lower animals; and, 2d, upon man, with a view to the possibility of the production of such a state of local anæsthesia, by the local application of chloroform or other anæsthetic agents, to individual parts of the body.

CHAPTER II.

ON THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE LOWER ANIMALS.

At a meeting of the Medico-Chirurgical Society of Edinburgh held on the 17th March, 1848, I took occasion to state that I had successfully chloroformed several of the lower animals,—annelida, crustacea, fishes, &c; that in some, especially in the common earthworm, (*Lumbricus terrestris*,) I had been able to produce local anæsthesia by applying the chloroform vapour locally; and had thus, at will, rendered anæsthetic, individual parts and portions of the worm, as the head merely, or the tail merely, or the middle part of the worm merely, the head and tail remaining unaffected. At the same time I recapitulated what I had stated at one of the February meetings,—that, in the human subject, local anæsthesia of a portion of the gums could be produced by rubbing the part with hydrocyanic acid. After the date of the above meeting, I was led to make some additional experiments upon the possible production of local anæsthesia in man; and in reporting the proceedings of the preceding sederunt of the Society, in the last number of the "Monthly Journal of Medical Science," the editor has stated, in a short foot-note, some of the results of those experiments upon the human subject, which I here intend to detail at greater length. (See "Monthly Journal," No. xci. p. 48.)

Nothing could be more curious or satisfactory than the experiments alluded to on the production of local anæsthesia by the local application of chloroform vapour to different parts of the body of the earthworm. The resulting degree of local anæsthesia in the part exposed is generally, in the course of two or three minutes, most complete, as regards both sensation and motion; in fact, after being sufficiently exposed, the chloroformed portion of the animal is quite flat and flaccid, does not move under any irritation, and can be doubled and twisted up upon itself like a piece of loose wetted cord. If the part paralysed by the chloroform is small, it will be dragged along by the movements of the other unaffected portions of the worm. Generally, in the course of a few minutes, it gradually regains

its powers of motion, and its irritability and contraction under stimuli.*

The easiest method of performing this experiment, is to place a small quantity of chloroform in the bottom of a tumbler, paste over the mouth of it a covering of paper, and make an aperture in this covering sufficient only to admit the portion of the animal that is to be chloroformed. When held in this position, the part of the animal below the paper and exposed to the vapour of chloroform, is generally thrown into violent movements for a minute or two before the state of anæsthesia supervenes. I have repeated the same experiments with the vapours of sulphuric ether, and of bisulphuret of carbon.

I have tried the same experiment with the same results upon the medicinal leech.

The results were, if possible, still more marked in another of the articulata that was submitted to experiment. A small myriapode or centipede (*Julus sabulosus*?), was rendered completely anæsthetic and motionless in the posterior segments of its body, by exposing that part alone for a few minutes to the vapour of chloroform. The five or six last rings of the centipede, with the suspended and motionless feet attached to them, were, for a short time afterwards, dragged about in a kind of paraplegic state, by the brisk and lively movements of the anterior and unaffected portion of the animal. The animal soon and completely recovered, each segment with its corresponding feet regaining its power of motion; and this in regular order from before backwards.

In other centipedes experimented upon, a small quantity of fluid chloroform was applied by a slight brush to the head, or to two or three of the middle rings of the animal, or to the tail, and always with the effect of anæsthetizing and paralyzing the part or parts only with which the chloroform came in contact. Sometimes when the head and anterior rings were alone touched, the animal after vainly trying to push its motionless head forwards, suddenly reversed the movements of all its limbs for a time, and dragged the paralysed head behind it. All of the centipedes experimented on recovered in a very short time from the effects of the chloroform.

By immersing the tail of the water-newt in chloroform vapour, the sensibility and motions of that part were rapidly destroyed, and returned in a few minutes afterwards. By a longer degree of immersion of the tail alone, the whole animal became anæ-

* These experiments become the more interesting in a physiological and toxicological point of view, when we recollect, that in the articulata the vascular system is general and distributed *longitudinally*, while both the nervous and respiratory system of this class of animals is in a great measure, segmentary and *transverse*, like the action of the anæsthetic.

thetic; and in several experiments it was found possible, but difficult, to give the animal in this way a fatal dose of the vapour.

The hind-leg of the frog becomes anæsthetic when exposed for four or five minutes to the vapour of chloroform. Immediately after, it drags the limb in progressing, and bears, apparently without feeling, pricking and irritation of it; but a galvanic current passed through it excites both sensation and motion. In one case, the motory power of the limb was not restored at the end of the third day. No effect appeared to result from keeping the hind-leg of this animal immersed in strong tincture of Indian hemp.

One hind-leg of a healthy, active rabbit, was confined in a large bladder containing the vapour of chloroform. At the end of an hour the common sensibility of the limb to pinching and squeezing was much impaired; but a current of galvanism passed through it produced crying and signs of pain. The power of moving the limb seemed unimpaired.

The hind-leg of a guinea-pig, similarly treated, exhibited the same phenomena at the end of an hour; but the anæsthesia was more complete. The skin of the leg was red and congested.

The posterior extremities and pelvis of a strong guinea-pig were enclosed in a bag containing the vapour of chloroform. At the end of an hour, no signs of pain could be extracted by pinching and squeezing either limb; and a current of electricity passed through a hind-leg evidently caused much less pain than when the same current was passed through a fore-leg. The whole hinder parts were very red and congested. The animal was also, in some degree, paraplegic, and dragged itself along by strong efforts with its anterior limbs.

In an interesting communication addressed, on the 7th of June, 1848, to the Yorkshire Branch of the Provincial Association, and published in the number of the *Provincial Journal* for June 28, Mr. Nunneley, of Leeds, has stated that he has produced complete paralysis of the individual limbs of frogs and toads, by immersion or exposure of them for a few minutes, to the vapour of chloroform; that by a similar but longer immersion, he had rendered the leg of the rabbit sufficiently anæsthetic to bear mutilation without pain; that he had immersed his own finger in anæsthetic fluids for about half an hour or an hour, and at the end of that time it was nearly powerless and insensible, nor was it entirely recovered for forty-eight hours; that in operating on the human eye he had rendered the organ anæsthetic by previously exposing it for about twenty minutes to the vapour of chloroform; and that, in his opinion, the action of

all, or of most anæsthetic agents might be produced locally by their local application, the sensorium being unaffected, consciousness retained, and the limbs not subjected to their influence being unaffected. This naturally leads us to the consideration of—

CHAPTER III.

THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE HUMAN SUBJECT.

EARLY in February, 1848, I was led to make the experiments to which I have previously alluded, relative to the artificial production of *local anæsthesia* in a portion of the human gums by friction with hydrocyanic acid, in consequence of being assured, on what I believe to be satisfactory evidence, that a dentist at Limoges, in France, M. Pernot, had the secret of extracting teeth with little or no pain, in consequence of previously applying some obtunding agent to the gums. I tried at the time a great variety of substances, in order to obtain this local anæsthesia, such as various ethers, bisulphuret of carbon, benzin, aconite, &c. Among all the agents employed, the effect of prussic acid was by far the most decided and complete; any part of the gum strongly rubbed by it, speedily became benumbed and insensible; but the resulting degree of anæsthesia was by no means sufficient for the purpose required. The results of these experiments were stated orally to the Edinburgh Medico-Chirurgical Society, at their meeting on the 16th February, 1848.

Before that date I had met with one instance in which local anæsthesia of the human hand had been produced in a young lady, in consequence of her accidentally holding in it for a considerable time a scent-bottle containing some chloroform. I tried at various times to reproduce a similar result in myself and in others, by keeping my hand wrapped in a napkin soaked in chloroform and other anæsthetic agents, but with little or, indeed, no decided success, till I used the vapour of chloroform raised by heat, the hand for the purpose having been immersed in a deep jar, into which a small quantity of chloroform was poured—the jar temporarily placed in a basin of water of the temperature of 130° or upwards, and the wrist or forearm being surrounded by handkerchiefs, so as to prevent the escape of the vapour.

A number of circumstances influence, however, the effect and the degree of the state of local anæsthesia; and as I have made a considerable variety of experiments, both upon myself

and upon others, in order to ascertain these points, I will shortly state the results. Let me premise, that in the experiments upon which the following results are founded, the hands of the same individual were generally held simultaneously in two different jars, differently arranged in regard to material or otherwise, in order to make two different and comparative experiments at the same time; and the relative degree of anæsthesia in each hand was ascertained during the experiment by pinching the fingers with the thumb nail, without removing the hand from the jar. After they were removed, these and other more severe measures were used with the same view, as tests of the degree of anæsthesia. Most of the experiments referred to were repeated and tried upon several different individuals. The general results were the following :—

1. When the hand is exposed to an anæsthetic vapour, it betimes presents the sensations of a limb benumbed, by compression of its larger nervous trunks,—the sensations, in fact, of partial or commencing paralysis. Usually after a short time, a glowing or burning feeling is perceived in the parts most exposed, and gradually there supervenes a sensation of thrilling and tingling, (like a limb asleep,) which deepens more and more. The skin turns red, and the hand at last becomes stiff and clumsy, and feels as if enlarged, and painful impressions, as pricking, pinching, &c., are felt less and less than in the other unexposed hand. After the hand experimented upon is removed from the vapour, it is generally half an hour or more before its usual normal feelings are quite restored. The nerves of motion are usually apparently as much affected as the nerves of sensation.

2. When the jar containing the chloroform or other anæsthetic fluid experimented upon, was immersed in warm water, so as to raise the vapour of the fluid more fully and quickly, the resulting anæsthetic effect was always greatly increased, both in rapidity and intensity.

3. The vapour of chloroform proved stronger than any other that was tried. When one hand, for instance, was immersed in a jar containing the vapour of sulphuric ether, and the other in a jar containing the vapour of chloroform, (both jars containing similar quantities, and being subjected to the same degree of heat,) the hand in the chloroform jar was both more speedily and more deeply affected than the other. In addition to the vapour of chloroform and ether, I have tried comparative experiments with the vapours of aldehyde, bisulphuret of carbon, iodide of methyle,* &c. The aldehyde had little or no effect

* Two or three months ago I began a series of experiments with the intention of testing the anæsthetic or other therapeutic effects of the various respirable compounds of amethyle, acetylene, &c. I was prevented from proceeding far in the inquiry, in consequence of the effects following the inhalation of the compound

of any kind. The iodide of methyle produced a very severe burning sensation, and left the hand intensely red and erythematous for a day or two afterwards, but with no marked anæsthetic influence. Among several of us that tried the vapour of bisulphuret of carbon, only one bore it for any length of time, (about an hour,) and in him it did not render the hand anæsthetic in any very appreciable degree; in myself and others the sensation of heat and burning became so utterly intolerable, as to force us to withdraw the hand in two or three minutes. Immersion of the hand for half an hour in Dr. Fleming's very powerful tincture of aconite,* or in a strong tincture of opium, and of Indian hemp, and in a strong solution of belladonna, produced no appreciable anæsthetic effect.

4. The hand, when plunged in liquid chloroform, is usually somewhat more deeply apathized than the other hand simultaneously held in the vapour of chloroform. This was the more general result with those who tried the experiment; but in some the chloroform vapour was as anæsthetic, or more so, than the liquid. Few persons can keep the hand for any adequate length of time in liquid chloroform; the sensation of burning becomes so intense and insufferable, as to force them to withdraw it in a very few minutes. On one occasion I held my hand for upwards of an hour in liquid chloroform without the part being more deeply apathized than it would have been by exposure to the vapour. One of my pupils, Mr. Adams, perseveringly retained his hand in the liquid chloroform for upwards of two hours: no great or very marked degree of local anæsthesia resulted. In these cases, in which the hand was long steeped in liquid chloroform, the sensations of burning returned severely, from time to time, as if in waves, during the experiment; and on removing it from the jar, the feelings of heat were temporarily aggravated. The normal sensibility of the parts speedily returned, and was completely restored within an hour or two in all, but the skin sometimes remained red and injected for a longer period; occasionally for several hours.

mentioned in the text, viz., iodide of methyle. I found it very powerfully anæsthetic, but dangerously so. After inhaling a very small quantity of it for two or three minutes, I remained for some seconds without feeling much effect; but objects immediately began to multiply before my eyes, and I fell down in a state of insensibility, which continued for upwards of an hour. I did not completely recover from the effects of it for some days.

* Some other forms or preparations of this and the other substances may possibly produce different results, as we know that aconite, when chewed, causes a numbness and tingling in the lips and tongue, lasting for some hours. See "Christison on Poisons," p. 2. In making this experiment with aconite, it appeared to me that the sensibility of the tongue, &c., was not decreased to pinching, &c., when the part was tingling from the effects of the chewing of the aconite.

5. The anæsthetic effect of chloroform, &c., is increased, both in rapidity and in degree, by immersing the hand, with the cuticle softened and moist. When one hand, for instance, is immersed without any preparation, and the other is prepared by being bathed and fomented for ten or twenty minutes previously, the latter almost immediately begins to tingle under exposure to the vapour, the dry hand not for some minutes. The degree of anæsthesia is also ultimately deeper in the moistened hand.

6. The degree of delicacy of skin in the person or part exposed to the anæsthetic vapour influences the result. In females I have seen the degree of the local anæsthesia of the hand that was produced, much greater and deeper than I could ever render it in the male subject. In applying the vapour of chloroform in small cupping glasses, &c., to different parts of the body, as the insides of the arms, &c., the resulting degree of local anæsthesia seemed, in a great measure, regulated by the tenuity of the skin of the part experimented upon. The skin of the axilla seems too tender to allow of the vapour being applied for a length of time sufficient to produce anæsthesia. One of my students, who kept both his lower extremities enveloped in strong chloroform vapour for three continuous hours, felt no appreciable local anæsthetic effect from it.

When strong chloroform vapour is locally applied to mucous surfaces, the attendant sensations of heat and smarting are too severe to allow of its sufficient continuance; at least, this is the result that I have obtained in applying it with small glasses to the inside of the lips, the tongue, and eye. Mr. Nunneley states, as we have already mentioned, that before operating on a difficult case of artificial pupil, he had applied for twenty minutes, a small quantity of the vapour of chloroform to the eye, by means of a small jar which accurately filled the orbit, with the effect of rendering the parts nearly insensible. Dr. Duncan and myself have repeatedly tried this experiment upon ourselves; but in none of the trials which we made, with the eye either shut or open, could we endure the burning action of the vapour upon the part above two or three minutes, and with no other result except always rendering the eye experimented upon red and injected, and suffused with tears.*

* I have tried the application of various anæsthetic gases and vapours to the vagina, in cases of vaginal irritation and neuralgia, but hitherto without much success. The stronger forms cannot be borne. I was induced to try them in consequence of the following curious statement regarding carbonic acid, published by Dr. Pereira, (*"Materia Medica,"* vol. i. p. 155.)—"A lady who had suffered a considerable time from some uterine affection, and had derived no relief from the treatment adopted, was advised to consult a physician in Italy, (Dr. Rossi.) After he had examined the condition of the uterus, he assured her there was no organic disease, but merely a considerable degree of irritation, for which he proposed to

7. The degree of anæsthesia produced in the hand by exposure of it to the strong vapour of chloroform, does not, in general, perceptibly increase after fifteen or twenty minutes. The same sensations continue if the hand is still retained in the jar; but an increased length of exposure does not, after a time, produce a corresponding increased degree of local insensibility.

But, finally and specially, I would observe that the degree of local anæsthesia produced in the human hand or skin, by exposing it to the local action of the vapour of chloroform, has never, in my experiments, been by any means so deep and complete in its character as to give the chance of annulling the pain of any severe operation, such as the deep incision, or amputation of a finger. As compared with the other non-exposed hand, the chloroformed hand is generally rendered, to a marked amount, *less* sensitive; but the insensibility is never, I fear, so entire and perfect as will save the patient from the pain of the surgeon's knife. In short, I altogether doubt, whether, in the human subject, we shall ever be able to reduce the knowledge of this possible production of partial local anæsthesia to any practical purpose. It is principally interesting in a toxicological and physiological point of view, and in relation to the doctrine of the mode of action of anæsthetic agents.* Its bearings are more upon the theory than upon the practice of anæsthesia.

These remarks relate to local anæsthesia as capable of being produced by the anæsthetic agents with which we are at present acquainted. Others may no doubt yet be detected, much more powerful than any we at present know,† and their local application carbonic acid as a sedative. This was done by means of a pipe and tube, communicating with a gasometer situated in another room. The patient obtained immediate relief, and although she had been obliged to be carried to the doctor's house, on account of the pain experienced in walking, she left it in perfect ease. On her return to England, she had a relapse of the complaint, and applied to Dr. Clutterbuck to know whether she could have the same remedy applied in London, in order to save her the necessity of returning to Italy."

* The distinction which exists between the structure and functions even of the skin in the lower animals, as compared with the skin of the human subject, perhaps sufficiently explains the differences in the degree of local anæsthesia, capable of being produced by the local application of anæsthetic vapours and fluids in the one and in the other. "In animals, (says Dr. Jackson, see *Amer. Cyclopædia of Pract. Med. and Surgery, &c.*, vol. i. p. 115,) whose skin is moist, and which possess a very delicate cuticle, cutaneous absorption is a constant and important function. Such are frogs, salamanders, and similar animals. The experiments of Edwards have established the skin in them to be entirely absorbing, and instrumental in their support."

† Perhaps some special modification in the application of electricity, galvanism or magnetism to the part to be locally anæsthetized, may be yet found capable of effecting this object. I have tried several experiments of the kind, but, as yet, without much success. The possibility of deep *local* anæsthesia existing in diseased states (as in colica pictorum, hysteria, hypochondriasis, &c.), without the general sensibility or consciousness being affected, is well shown in a late interesting essay of M. Beau, in the "*Archives Générales de Médecine*" for January, 1848. He relates one case of an insane patient who broke his fibula, and

cation may enable us to effect the local anæsthesia desired. At the same time, this consummation, even, seems doubtful; for, perhaps, any agent possessing a deeper and more rapid anæsthetic local power, would, by absorption, affect the system generally, and it may be, dangerously, before complete local insensibility of a part could be effected. Some time ago, in attempting to produce local anæsthesia in my hand, by exposing it to the vapour of hydrocyanic acid, ere the hand was much, or very perceptibly benumbed, I began to feel the constitutional effects of the poison; my respiration became irregular, and I felt giddy and faint, when my assistant removed my hand from the jar. All due care was taken to prevent me breathing any of the vapour, and I sat during the experiment in a current of air. I felt the benumbing influence of the acid extending from the hand upwards along the arm a minute or two before the experiment was stopped.

In addition to the liquids and vapours experimented upon, I have tried long immersion of the hand in various gases, as carbonic acid and common coal gas, (both of them powerful general anæsthetics when inhaled,) without any effect. Chaptal, however, alleges that he had felt the limbs plunged in carbonic acid much benumbed; and Collard de Martigny* found that when the general surface of his body was immersed in carbonic acid, (arrangements being made so that none of it was breathed,) giddiness, ringing in the ears, and the other symptoms produced by the action of this gas, supervened in eight or ten minutes, proving that it was absorbed. Davy† felt the premonitory exhilaration of nitrous oxide gas by exposing the surface of his body to it in the same way.

Chaussier‡ inclosed the leg of a dog in a bag containing sulphuretted hydrogen, and found that he could in this way, in a few minutes, induce the poisonous effects of this gas; and similar experiments were repeated by Lebkuchner§ and Nysten|| and my friend Dr. Madden¶ on the rabbit, &c., with similar results. I have held my hand inclosed in a bag filled with the constantly renewed vapour of sulphuretted hydrogen for about half an hour, without feeling any local effects.

continued to walk about without pain until the limb became inflamed and gangrenous. It was then amputated, and still without the patient apparently suffering in any degree during the operation.

* "Archives, Génér. de Méd.," vol. xiv. p. 209.

† "Researches on Nitrous Oxide," 1800, p. 485.

‡ Sedillot's "Journal de Méd.," vol. xv. p. 25.

§ Diss. Inaug. utrum per viventium adhuc animalium membranas materiæ ponderabiles permeare queant, p. 10, &c.

|| Nysten. "Recherches Physiologiques," p. 137, &c.

¶ See his excellent "Experimental Inquiry into the Physiology of Cutaneous Absorption," p. 133.

And even supposing that we could, by any means, so benumb the sensibility of a part to be operated upon as to render it anæsthetic, I doubt much if this state of local anæsthesia would place the parts in a condition at all favourable for being subjected to operation. It is quite possible, nay, probable, that other more powerful agents than those which I have experimented upon, may, by their local application, produce a greater and deeper local anæsthesia on the human subject than I have yet witnessed; but all, or almost all, of those that I have used, have, along with the anæsthesia, led to such a congested and injected condition of the part, as to give every likelihood, both of a greater tendency to hemorrhage at the time of operating, and of a greater tendency to inflammation in it immediately afterwards.

Other and more simple agencies than any which I have mentioned are capable of producing a certain amount of local anæsthesia. A mere exposure of the hand, for instance, to great changes of temperature, either in the way of increase or diminution, has the power of deadening the sensibility of the part. I have tried, and seen others try, to hold the hand, with a view to this, immersed in broken ice, or dipped in ice-water, and with the effect of inducing a degree of anæsthesia in the part, as deep, if not deeper, than exposure of the same part for a greater length of time to the local action of chloroform would have effected. In his admirable and classical "*Lectures on Inflammation*," the late Dr. Thomson remarks, (p. 617,) that "the sensibility to external impressions of the parts exposed to cold, is always more or less impaired, and the diminution in the sensibility of the nervous system seems to admit of degrees, from the slightest perceptible numbness, to that of the most complete insensibility." But he also correctly adds—"This diminution, however, of the sensibility to external impressions, is not unfrequently accompanied with severe degrees of pain." In fact, in making the experiments to which I have above adverted with the pounded ice, few of us could allow the hand to be retained in it above two or three minutes, in consequence of the intense pain which ensued; but still that brief period produced, as I have said, a most distinct and well-marked degree of local anæsthesia.

Keeping the hand immersed in water, of as high a temperature as it can conveniently bear, has also the same effect of rendering it, in five or ten minutes, partially anæsthetic and benumbed, as compared with the other unexposed hand; and this without the pain and suffering connected with the other extreme of temperature.

It is not my object here to inquire in what relative diseased

or other conditions of a part, heat or cold are respectively calculated to act anæsthetically. But I would beg to make this general remark, that the action of the above, and of other applications, which we use to inflamed, burned, neuralgic and pained surfaces and parts, (such as preparations of opium, conium, aconite, belladonna, tobacco, &c., &c.,) still require to be studied, which they have not yet been, in another and a new light,—viz., as LOCAL ANÆSTHETICS. Therapeutical writers will, I believe, sometimes consider and describe them in this novel point of view; and when attention comes to be directed to them with this object, some new facts and precepts may be elicited that will enable both the physician and surgeon, to exhibit and apply local anæsthetics with more science, precision, and success, than hitherto.

The experiments and observations which I have detailed in the preceding pages, perhaps entitle us, in the present state of our knowledge, to draw the following conclusions:*

1. In animals belonging to the class of Articulata, complete local and limited anæsthesia can be produced by the local and limited application of the vapour or liquid of chloroform to individual parts of the body of the animal.

2. In Batrachian reptiles, the tail, or an individual limb, can be affected in the same way with local anæsthesia, by the local application of the chloroform; but, in addition, general anæsthesia of the animals usually results in a short time, in consequence of the chloroform absorbed by the exposed part coming to affect the general system.

3. In the smaller mammalia a single limb, or even the whole lower or pelvic half of the body, can be rendered anæsthetic by local exposure of these parts to the influence of chloroform.

4. In the human subject partial, and, perhaps, superficial, local anæsthesia of a part, as the hand, can be produced by exposing it to the strong vapour of chloroform; but the resulting degree of this local anæsthesia is not sufficiently deep to allow the part to be cut or operated upon without pain.

5. Any agent possessing a stronger local benumbing, or an anæsthetic influence, would probably be dangerous, by its acting

* Through the kindness of Professor Balfour I have had various opportunities of trying the effect of chloroform vapour upon the sensitive plant, (*Mimosa pudica*.) When the vapour was either too strong or too long continued, the plant was destroyed. When it was weaker and applied only for a few minutes, the leaflets in some plants closed as when irritated, and did not expand again for an unusual length of time. In other plants under exposure to the chloroform vapour, no closure of the leaflets took place, and, in a few minutes, the plant became so anæsthetized, that the mechanical or other irritation of the leaflets or stalk did not produce any of the common movements; nor did their irritability become restored for a considerable time afterwards.

too powerfully on the general economy, before the local anæsthesia was established to a depth sufficient for operating.

6. Artificial local anæsthesia, from any known anæsthetic agents, seem objectionable in any part intended to be operated upon, in consequence of the vascular congestion and injection which attend upon and accompany this local anæsthesia.

7. There are few operations in which there is not previously a local broken surface; and the application of chloroform, &c., to such a surface, would be far too painful to be endured, no small degree of suffering sometimes arising from even the exposure of the unbroken skin to their action.

CHAPTER IV.

ANSWER TO THE OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY, ADDUCED
BY PROFESSOR MEIGS OF PHILADELPHIA.

Starbank, by Edinburgh, 1st August, 1848.

MY DEAR SIR,—

A few days ago, I saw your excellent epistle to me on the use of Anæsthesia in Midwifery, extracted in an abridged form, from the Philadelphia Medical Examiner of March last, into the London Medical Gazette and Lancet. It reminded me, that, amid other avocations and work, I had hitherto indolently omitted to answer the objections contained in your able and kind letter. And I feel that I am the more to blame for this neglect, on one account,—namely, that as in your own country, so also in ours, there are few or no living obstetricians, whose opinions and name carry, and deservedly carry, more weight with them than yours. Be so good then, as bear with me now for a few minutes, while I endeavour to state in what respects I am inclined to demur to your arguments against anæsthetic midwifery.

On reperusing (as I have just done) your esteemed letter, it appears to me that in it, you ground your opposition to the adoption of anæsthesia in midwifery, upon four or five different arguments, although you do not specialize them. I shall notice each of these arguments separately. You have not placed them in any particular order. I shall begin first with the one which you placed last.

1. *You object to anæsthesia in deliveries, requiring “chirurgical intervention,” and especially in forceps operations, on the ground that the sensations of the patient, afford us our best aid for the introduction of the instrument.*

In order to introduce the forceps with the greatest safety to the mother, you state that—(to quote your own words)—“the best guide of the accoucheur is the reply of the patient to his interrogatory, ‘Does it hurt you?’ The patient’s reply, ‘Yes, or No,’ are, (you observe,) worth a thousand dogmas and precepts. I cannot therefore (you continue) deem myself justified in casting away my safest and most trustworthy diagnosis, for the questionable equivalent of ten minutes’ exemption from pain, which, even in this case, is a physiological pain.”

In answer to this novel objection, you will excuse me when I say (for I say it most conscientiously) that I think every man who ventures to use the forceps, in any midwifery case, ought to know the anatomy of the parts implicated, a thousand fold better than you here presuppose. You would have the ac-

coucheur guide his instrument, not so much by his own anatomical knowledge, as by the feelings and sensations of his patient. In this, as in other points, relative to any novel question in practice, we can often, it appears to me, best perceive the soundness or unsoundness of our views upon it, by considering and contrasting them with our established views on other analogous questions, regarding which, the opinions of the profession have been long ago fixed and determined. Now, what would the surgical world, at this time of day, think of an operator, who, in making a ligature of a large artery, such as the humeral, placed his chance of discriminating the attendant nerve from the blood-vessel, which he wished to tie, by appealing not to his own anatomical knowledge, but to the feelings of his patient, as he touched the suspected structures. "Does it hurt you?—Yes, or No?" Would our surgical brethren not denounce and decry the capabilities of any man who, in operating, required to have recourse to such imperfect and incompetent means, for his anatomical direction and diagnosis? Would it be right and moral in a surgeon to deny to his patients the advantages of anæsthesia, in order that their sensations and sufferings should make up for his want of anatomical and operative knowledge?

But, in saying this, do not, I pray you, for one moment suppose that I fancy that the argument which you adduce betrays any want whatever of the highest degree of operative skill on your part. Nothing could be further from my thoughts. And, to confess the truth, I do sincerely believe that you yourself, while using the forceps, do not require to have recourse to any such rude rule as you here propound,—and that, in fact, the rule itself, and the objection to anæsthesia in operative midwifery which it contains, is an *afterthought* on your part, which has only sprung up since the practice of anæsthesia was proposed. For, in looking over the excellent precepts which you have given, relative to the use of the forceps, in the valuable work on Midwifery which you published a few years ago, (viz., the Philadelphia Practice of Midwifery,) I find no trace or mention whatever of such a rule as you have quoted above, in your letter to me. If that rule really formed, as you now state, the "safest and most trustworthy" guide in the operation, you would certainly have at least noticed it, or alluded to it in some way. In the precepts which you laid down in your work, you would assuredly not have forgot that one rule, which, you say, is worth a "thousand other dogmas and precepts." And it would, I think, have been only the more incumbent upon you to have mentioned it, seeing that all other authors omit the notice of it.

I feel assured, that when you come to reconsider "dispas-

sionately" your opinions regarding the non-employment of anæsthesia in operative midwifery, you will alter these opinions. And when you come to employ anæsthesia in actual practice, in cases in which the forceps are used, you will find that, instead of impeding the application of instruments, the anæsthetic state very greatly facilitates it. It enables you to guide the forceps far more safely to their destination, because it enables you without any pain to the patient, to introduce your fingers for this purpose, far more deeply between the head and maternal structures, than you could do if the patient were awake, and in her usual sensitive state. You yourself state, in your published work on Midwifery, that care should be "taken to direct the point (of the forceps) by the two fingers, as far as they can reach," (p. 300.) "If (you again observe) any difficulty occurs in getting the second blade forward enough, the two left fingers that are guiding it will serve to guide it edgewise into the proper position." Now, the state of anæsthesia, I repeat, gives you (as I have several times found) the power of fulfilling these and other most important rules, to an extent that never can be attained without it; and I am sure you will find them worth any "thousand dogmas and precepts" derivable from the mere sensations of the patient.

Besides, these sensations, or rather the expression of them, would constantly betray you if you *did* place any dependence upon them. Under the same amount of pain, scarcely any two women would give you exactly the same expression of suffering. What one woman would loudly complain of, another would declare to be nought.

Before interfering instrumentally with the forceps, the labour has generally been allowed to endure for twenty or thirty long hours. After a poor patient has undergone such a protracted ordeal of pain and suffering, her mind is not, I fear, in general in a very fit state to guide the operator by her sensations or directions.

At page 302 of your published work on midwifery, you state that when the forceps are used, the patient's mind is naturally wound up to a state of great anxiety; "it is strained (you observe) to the highest tension, by the mere thought that she is under the operation." Now, putting entirely out of view for the moment, the propriety of our saving our patients the increased corporeal agony attendant upon instrumental delivery, is it not, let me ask, our right and our duty as medical men, to save her, as we can do, from this trying state of mental anxiety at the time of operating? In most cases, she will have been suffering and struggling on for many hours previously. Why then thus needlessly and greatly intensify both her mental anxieties, and physical sufferings at the time of our instru-

mental interference, when her strength, alike of mind and body, are perhaps little calculated to bear any increase of suffering, and above all, when the resources of our art furnish us with simple and certain means of saving her from the unnecessary endurance of the one state and of the other?

But, in instrumental delivery, besides greatly facilitating the application of the forceps, and relieving the patient from enduring the pains of the operation, and that "highest tension" of mind which is present during it, the state of anæsthesia saves her, I believe, also in a great measure, from the effects of the shock of the operation, and thus gives her a better chance of recovery. If we omit it, we omit, I believe, not only a means of saving her from the sufferings attendant upon the operation, but a means of saving her from some of the dangers attendant upon it. When first publishing on the subject of anæsthesia in midwifery, in February 1847, I offered one or two observations on this point, which subsequent surgical statistics have amply fulfilled. In allusion to some cases of operative delivery, which I recorded, I observed:—"The cases I have detailed, sufficiently show its value and safety, in cases of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtlessly be. But in modifying and obliterating the state of conscious pain, the nervous shock otherwise liable to be produced by such pain,—particularly whenever it is extreme, and intensely waited for and endured,—is saved to the constitution, and thus an escape gained from many evil consequences that are apt to follow in its train."

The observations which I have hitherto made, refer entirely to your opinion of anæsthesia in instrumental delivery. But,—

2. *You object to anæsthesia in natural labours, because you hold that the pain of natural labour should not be annulled, and that it is calculated to promote the safety of the mother.*

You regard, you say, "the pain of a natural labour as a state not by all possible means and always to be eschewed and obviated,"—"a labour pain being (you declare) a most desirable, salutary, and conservative manifestation of life-force."

In the above expressions you make no distinction between the two separate and distinct elements of which a so-called labour pain consists, viz.: 1, the contractions of the uterus, and 2, the sensations of pain resulting from these contractions. If you apply the language I have quoted to the first of these elements, the uterine contractions (and which contractions are not annulled by anæsthetics), I decidedly and entirely agree with you. If you apply it, however, to the sensations of pain produced by the uterine contractions, (and which sensations are

annulled by anæsthetics,) I most decidedly and entirely dissent from your opinion.

In your work on midwifery, you make, correctly, the important distinction to which I refer. You state (p. 148) that "the word (labour) is highly expressive of the violent and painful struggles and efforts of the woman." You add that "the essential element of labour is the contraction of the muscular fibres of the womb." And, at page 303, in speaking of the strength of these uterine contractions, you observe, "let it be well borne in mind that the expulsive powers of the womb are enormously great." In more than one place in your work you allude to the intensity of the sensations of pain (the pangs and agonies of travail, as you term them, p. 155); and at page 153 you speak of the "painful sensations" of the mother in the last part of labour as so great in degree "as to be absolutely indescribable and comparable to no other pain." In your still later work on Female Diseases, speaking of these pains,—the pains of parturition,—you observe, "Men cannot suffer the same pains as women. What (you continue) do you call the pains of parturition? There is no name for them but Agony."

The muscular contractions of the uterus form, you say, the "essential element" of labour. In that opinion you and I are at one, and further I quite agree that this cannot safely be "eschewed and obviated" in natural labour; nor are they "eschewed and obviated" under the proper use of chloroform.

But the pain, the second element, is a non-essential element in the process. It is non-essential because, 1, labour, that is the uterine contractions, are occasionally though very rarely in the course of practice, seen to accomplish the full expulsion of the child with little or no pain; 2, in whole tribes of the human race, as in some of the black tribes, comparatively little or no pain seems to be endured, if we may believe various authorities; and 3, hundreds of women have, during the last year, been delivered with perfect safety, but without any pain, while placed under the influence of anæsthetic agents.

I hold the pain to be *non-essential*, and I protest against the truth of your opinion that "the pain of a natural labour is a state not by all possible means to be eschewed and obviated." On the contrary, I maintain that we omit and forego a mighty part of our professional duties whenever we forget the axiom of Bacon, that "it is the office of a physician not only to restore health, but to mitigate pain and dolours." And if, as medical men, we are called upon to mitigate and remove pain of any degree in our fellow-beings, we are surely called upon to mitigate and remove those "pangs and agonies of travail" (as you term them) which in degree are, in your own language, "abso-

lutely indescribable and comparable to no other pain," — "pains for which there is no other name but Agony."

In your practice, you, like other medical men, constantly use measures to mitigate and relieve the pains of headache, of colic, of sciatica, of pleurodyne, of gout, rheumatism, and all the other innumerable "dolours" that flesh is heir to. Like other physicians, you deem it, I doubt not, your duty to wield the powers of your art in order to free those that submit themselves to your medical care from these and from other similar sufferings. But if it is right for you to relieve and remove these pains, why is it not right for you also to relieve and remove the pains accompanying the act of parturition? I cannot see on what principle of philosophy, or morality, or humanity, a physician should consider it his duty to alleviate and abolish, when possible, the many minor pains to which his patients are subject, and yet should consider it improper to alleviate and abolish, when possible, pains of so aggravated a character, that, in your own language, they are "absolutely indescribable and comparable to no other pains," pains for which there is "no other name but Agony."

3. *You object to anæsthesia in natural labour, because you deem the pain of natural labour "a physiological pain."*

"The sensation of pain in labour is (you observe) a physiological relative of the power or force," and "to be in natural labour is the culminating point of the female somatic forces."

Now, for the reasons that I have already stated, I entirely doubt if we should look upon the severe sensations of pain endured by our patients as truly "physiological," for, as I have just stated, they are *not* essential to the mechanism and completion of the process in the white races of mankind, and they are absent, to a great degree, in the black. The severity of them could, I think, be easily proved to be the result of civilization, and, as I believe, of that increased size of the infantile head which results from civilization. Parturition is always physiological in its object, but not in some of the phenomena and peculiarities which attend upon it in civilized life.

But, waiving this point, or the discussion of it, let me state, that even if I allowed all the intense pains of parturition to be "physiological pains," I cannot conceive that to be any adequate reason for us not relieving women from the endurance of them. Because nature has fashioned any particular physiological function in any particular manner, that, I opine, is no reason why the science and art of civilized life should not, when possible, alter and amend its workings. If it were improper for us, for instance, to intermeddle with the functions of the hair of the head, or of the skin generally, then all hats and

other coverings for the scalp, all clothings and coverings for the body, should be at once abandoned and unconditionally condemned. If it were improper for us to alter and amend the functions of the eye, then all optical glasses, the telescope, the microscope, &c., must be thrown aside. And indeed not later than the 17th century it was held and argued so in England. For, in his history of the first beginning of the Royal Society of London, Sprat tells us that it was generally believed that this "new experimental philosophy (namely, the philosophical papers laid before the society) was subversive of the Christian faith," and many, he adds, mortally hated the newly invented optical glasses, the telescope, and the microscope, as *atheistical inventions*, which perverted our organs of sight, and made everything appear in a new and false light (D'Israeli). You argue as if we should not use means to eschew the pains of parturition *because* that pain is physiological. When Columbus first discovered your mighty American continent, a large portion of the inhabitants were unprovided with any kind of dress or covering. "To most of them," says Robertson, "nature had not even suggested any idea of impropriety in being altogether uncovered." And I do think that men living in such a state, could, against the fashion of dressing, use with far greater propriety and consistency than you or me, your own argument against anæsthetics in labour. Chloroform and ether should not be used in labour (you argue), because the pain against which they protect us is natural and physiological. No kinds of clothing or dress should be used (the original Americans might have equally argued), because the cold or heat against which they protect us are natural and physiological.

I have a letter lying before me on the subject of anæsthetics in midwifery, by a very highly and very justly esteemed teacher of midwifery in Dublin. "I do not (he writes) believe that any one in Dublin has as yet used ether in midwifery; the feeling is very strong against its use in ordinary cases, and merely to avert the ordinary amount of pain which the Almighty has seen fit,—and most wisely we cannot doubt,—to allot to natural labour; and in this feeling I heartily and entirely concur."

The argument thus used, and so very well expressed by my Irish correspondent, is one which has been often adduced and repeated during the course of the past year. Some minds at first gave immense weight and importance to it. For my own part I must confess that I never could view it as possessing any great force. Look at it as applied to any other practice which happens to be sufficiently old and established; and then we will see it in its true import. Supposing, for example, it referred to the *first* introduction of carriages into use; it would then read thus: "I do not believe that any one in Dublin has

as yet used a carriage in locomotion ; the feeling here is very strong against its use in ordinary progression, and merely to avert the ordinary amount of fatigue which the Almighty has seen fit,—and most wisely we cannot doubt,—to allot to natural walking ; and in this feeling I heartily and entirely concur.”

Nay, this frequently repeated argument against new innovations becomes not only, I think, ridiculous, but really almost irreverent, when we look far backward into the march of civilization, and apply it to any practices that are so very long established as to be very antiquated, and with which, therefore, the human mind has been long and intimately familiarized. Some one, but who I cannot pretend to say, no doubt first introduced the practice of wearing hats or bonnets or some covering for the head. Supposing this practice, however, stoutly resisted, as doubtlessly it was at first, then the argument of my Dublin friend against this innovation would read somewhat as follows: “ I do not believe that any one in Dublin has as yet used a hat to protect his head ; the feeling here is very strong against its use in ordinary weather, and merely to avert the ordinary amount of wetting and cold which the Almighty has seen fit,—and most wisely we cannot doubt,—to allot to mankind ; and in this feeling I heartily and entirely concur.” Some day a canal will, in all probability, be made through the Isthmus of Panama. It has, you are well aware, long been proposed to cut one ; and there and thus unite the Atlantic and Pacific Oceans. When it was proposed in the 16th century, a priest of the name of Acosta brought forward the following reason against it. “ I am (said he, writing in 1588) of opinion that human power should not be allowed to cut through the strong and impenetrable bounds which God has put between the two oceans, of mountains and iron rocks, which can stand the fury of the raging seas. And, if it were possible, it would appear to me very just, that we should fear the vengeance of Heaven for attempting to improve that which the Creator in his almighty will and providence has ordained from the creation of the world.” The arguments which are here brought forward by the earnest Spanish priest against man meddling with and altering the impediments to navigation caused by the natural mechanism of the Isthmus of Panama, are essentially the same as those lately brought forward against men meddling with and altering the agonies caused by the natural mechanism of parturition in the civilized woman. We can all, perhaps, at this time of day, see through and smile at the character of the old priest's argument with regard to the supposed impropriety of changing and cancelling, if possible, the natural obstruction produced by any isthmus. Some years after this, perhaps, our descendants will equally see through and smile at, the analogous modern argument in regard to the supposed im-

propriety of changing and cancelling, when possible, the physical suffering produced by a physiological function.

The truth is, all the tendencies of man in a civilized state of society, are to intermeddle with and change, and, as he conceives, improve the action of almost every function in the body. And each such improvement has at the time of its introduction, been, like the practice of anæsthesia, very duly denounced as improper, impious, &c., &c. I might refer to numerous such cases. Let me cite only one example. The human fingers are admirably constructed by our Creator for the function of seizing and lifting objects. The late Sir Charles Bell wrote a whole octavo volume—a Bridgewater Treatise—on the mechanism of the human hand as beautifully adapted for this and other functions. In the reign of the earlier Stuarts, forks were introduced from the continent to assist our hands in the act or function of seizing and lifting the divided portions of meat, &c., that we wished to eat. But this was a very sad and uncalled for innovation upon the old and established physiological functions of the human fingers; and, at the time, it was as loudly opposed and decried as the modern employment of anæsthetics in aiding the physiological function of human parturition. D'Israeli tells us that the use of forks was so much reprobated in some quarters, that some uncleanly preachers denounced it "as an insult on Providence not to touch our meat with our fingers." Nature herself has provided us with fingers of flesh and bone and nerve, and consequently is it not unnatural and impious in man to attempt, in his human pride and arrogance, to substitute for these, artificial metallic fingers of silver and steel?

I repeat,—all our tendencies and workings in the present state of civilization, are attempts to intermeddle with and change and improve the action of almost every function in the economy. And assuredly if we use means in regard to the function of parturition with the view of ameliorating and abolishing the unnecessary, but, as you call them, "absolutely indescribable" pains that attend upon it, we would be doing nothing more than what you and I and all of us are ever doing in relation to most of the other natural or physiological functions of our own bodies.

Let me illustrate this last remark by one more example, for, as I have already said, it is only in this way that we can properly judge of the soundness or unsoundness of our views of novel points in theory or practice. You are well aware that the act of parturition has been often familiarly compared, as the late Professor Hamilton expressed it, "to the toils of a journey," and like it divided into stages. "The sufferings of the mothers (says he) have been in most languages compared

to those of travellers." Now let us for a moment continue this natural simile between the function of parturition and the function of progression. You maintain that "labour is the culminating point of the female somatic forces." One of the most illustrious Presidents of your great American Republic—Thomas Jefferson—makes in his memoirs a remark of precisely the same import regarding walking or progression. He describes the act of walking, but not exactly in the same words, as the kind of "culminating point of the human somatic forces."*

Few or none, perhaps, will question the abstract truth of Jefferson's observations on this point. But, because walking or progression is a "physiological" function, and the practice of it is reputed salutary, would this be, with you, a proper and sufficient reason for never setting aside or superseding in any way this "physiological" state, in the same way as you insist, on the same grounds, that the physiological pain of labour should not be set aside or superseded. Because progression is a natural condition, would this be any adequate reason for your medical advisers adopting your own arguments against anæsthesia in midwifery, and insisting upon this, that, the next time you travelled from your own city of Philadelphia to the cities of Baltimore or New York, you should walk the distance on foot instead of travelling it by railway or other conveyance. What opinion would you form of the judgment of any medical adviser to whom you entrusted your own health, if, on going next time to the New York or Baltimore railway station, he should gravely and solemnly repeat to you as his patient what you tell your midwifery patients, and, in your own language, advise you to try to accomplish the intended journey on foot as (to quote your own words) "a desirable, salutary, and conservative manifestation of life-force." And yet this would really be nothing more than making your *argumentum ad feminam* an *argumentum ad hominem*.

You state, in a passage which I have already quoted, that even the agony accompanying instrumental delivery by the forceps is a "physiological pain." I do not, I confess, see why the suffering attending the use of the forceps, when the head is impeded by any cause of obstruction, should be regarded as a "physiological pain," any more than the suffering attending

* Since writing the above, I have turned up Jefferson's Memoirs to get his own words. "Walking (says the American President) is the best possible exercise; habituate yourself to walk very far. The Europeans (he continues) value themselves on having subdued the horse to the uses of man; but I doubt whether we have not lost more than we have gained by the use of this animal. No one has occasioned so much (as the horse) the degeneracy of the human body. Our Indians go on foot nearly as far in a day for a long day as an enfeebled white does on his horse; and he (the Indian) will tire the best horses."—(Memoirs, vol. i. p. 287.)

the use of the catheter in obstruction from the prostate gland or other morbid conditions of the urethra should be regarded as a "physiological pain." They are both operations intended to remove the natural contents of the respective viscera, when their operative removal becomes necessary.

But let us waive this point and return again to the analogy between the functions of progression and parturition. Suppose you plead with your medical adviser that, instead of insisting on your going on foot, they should allow you *for once* to take advantage of artificial assistance, and proceed on your journey from Philadelphia to Baltimore or New York by railway, because you were unable to walk the distance in consequence of being incapacitated by a rheumatic knee, or a sprained ankle, or an inflamed or blistered toe, and they replied to you that you should not care for this, but still proceed and suffer, because the pain you might thus suffer was (to use again your own language) still only a "physiological pain." Would that argument be any adequate philosophic consolation under the endurance of your suffering? or would you not laugh at the logic of your medical adviser, and take your seat in the railway in spite of his doctrine? And I have a fancy that betimes, in midwifery, patients *will* learn to adopt exactly the same line of practice under the analogous circumstances, and think and act too exactly in the same way.

4. *You object to anæsthesia in labour, because the mother, in escaping by it from the "pangs and agonies of labour" may, in a few rare cases, be thus made to encounter danger to her own life.*

"Should I (you observe) exhibit the remedy for pain to a thousand patients in labour, merely to prevent the physiological pain, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the life or the death of one in a thousand in a questionable attempt to abrogate one of the general conditions of man?" Let me add that I have seen this argument of yours already repeated from your letter, and strongly insisted upon by the opponents of anæsthesia in this country.

And, indeed, in a new practice, such as that of anæsthesia, and with which the mind is yet not at all familiarized, the above forms one of that kind of apparently strong statements, which it is impossible to answer directly, or, indeed, by any other way than by taking, as I have already said, a corresponding illustration and simile from some other matter with which the mind is already familiarized. Let us for a moment longer, then, adhere to the familiar comparison which I have already

taken up, under the last head, between the physiological function of human parturition, and the physiological function of human progression. Suppose, then, that you and I were standing at the Philadelphia station on the first day of the opening of the railway to Baltimore or New York. I wish the passengers to Baltimore or New York, or the shorter and intermediate stations, to proceed thither by railway; but you argue with them, like President Jefferson, that "progression is the culminating point of the human somatic forces," and that "walking is a desirable, salutary, and conservative manifestation of life-force," and that progression being a "physiological function," and fatigue a physiological pain, they ought to proceed on foot. I say "No." Place yourself in a railway carriage, and thus eschew and obviate all the great fatigue and useless over-exertion of foot travelling. Then comes that answer and argument of yours which I have quoted, and which runs as follows. "But should I exhibit, sir, the remedy for fatigue (*a railway carriage*) to a thousand travellers, merely to prevent the physiological exertion and fatigue of walking, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the death of one in a thousand in a questionable attempt to abrogate one of the general conditions of man,—viz., his power of progression by walking?"

I shall not stop to inquire whether among our supposed lady passengers or patients (uninured, as most of them are, either to long pain or long walking) more than one in a thousand would not be worn out and destroyed by taking the journey on foot. A less proportion, I believe, would be found to be ultimately destroyed by the perils and dangers of the journey by railway than by the exertion and fatigue of the journey on foot, and the walk would shake and damage, both temporarily and permanently, many more constitutions than the railway carriage. I have a firm conviction that, on the great scale, there would be found a more absolute saving both of human life and of human health by adopting the means invented by art than the means provided by nature. And I most firmly believe that yet a similar difference will be found to hold good between the two corresponding practices of allowing women to pass through labour afflicted with all their usual physiological "pangs and agonies," and carrying them through that process without their being subjected to the endurance of these pangs and agonies.

But I proceed to remark, that if your supposed theory with

regard to the function of parturition were carried out in regard to the other functions of the human body, it would produce a vast and mighty revolution in the practices of civilized life. Follow it out, for instance, with regard to any one of them, as, for example, with regard to the one we have already spoken of, viz., progression, and see what would be the results. Ever and anon our newspapers contain paragraphs, telling us of one or more human lives being lost by collisions on railways, explosions of steamboats, upsettings of stage-coaches, &c. Consequently, according to your doctrine, the featherless biped pedestrian man, should no longer, when travelling, fly in railway cars, ply in steamboats, ride in coaches, &c., for these are evidently all so many questionable attempts to abrogate what you call "one of the general conditions of man, viz., his original pedestrianism."

In the great government and police of nature, disease and death are among the most certain "general conditions of man." If your theory were true, the practice of medicine itself should, I fear, be at once and summarily abandoned, for perhaps, in your own language, it is, at best, a questionable attempt to abrogate one of the general conditions of man, and I am sure you will agree with me, that in this "questionable attempt" human lives are often lost from the mistakes or the passiveness, or the want of knowledge and skill on the part of the physician. In England and Wales, in 1840, there were, according to the returns of the Registrar-General, above 100 persons publicly and officially reported as having died from the effects of one drug alone, opium. But would this be any reason, or any ground of reason, for abandoning in medicine the use of opium, perhaps, in itself, the most valuable of all the remedies in our pharmacopœia? Would this be any adequate argument for refusing to relieve, by a dose of opium, the next appropriate case of pain that you are called to? Or because chloroform or ether, in a very rare case, now and again produces deleterious or even fatal consequences, should we refuse, in a thousand other persons, to mitigate and annul their agonies by its use?

In your esteemed letter to me, you quote some remarks from the celebrated old work—Raynald's *Birth of Mankind*, the first book on Midwifery printed in English. Look at the Prologue to the work. It is excellent in reference to the very matter we are discussing, viz., whether the rare accidents, from abuse or otherwise, to which any good gift may occasionally subject those who use it, should be a reason for repudiating the general use of that gift. "There is not anything (says Raynald) so absolute and perfect, but by the occasion of the abuse thereof at one time or other, may and doth ensue greater damage and danger to mankind." He instances fire and

water, "two righte necessary elements to the use of man, without the which he could not live," yet sometimes "by fire hath bin consumed and devoured whole cities and countries; by water swallowed and drowned infinite men, shippes, yea and whole regions. Againe, (he continues) meate and drinke, to the moderate users thereof, doth minister and maintain life; and contrary, to the unmeasurable and unsatiate gourmands and gluttons, it hath full many times brought surfeet, sicknesse, and at the last, death. . . . But (he argues) should men, for the avoyding of the aforesaid inconveniences, and for the reasons above said, condemne and banish fire and water, or forsake their meate and drinke? No, *it were but madnesse once to think it.*"

Before passing from these, your supposed dangers of anæsthetics, let me add two remarks,—1st. I do believe that if improperly and incautiously given, and in some rare idiosyncracies, ether and chloroform may prove injurious or even fatal,—just as opium, calomel, antimony and every other strong remedy and powerful drug will occasionally do. Drinking cold water itself will sometimes produce death. "It is well known," says Dr. Taylor, in his excellent work on Medical Jurisprudence, "it is well known that there are MANY cases on record, in which cold water, swallowed in large quantity, and in an excited state of the system, has led to the destruction of life." (p. 8.) Should we, therefore, never allay our thirst with cold water? What would the disciples of Father Matthew say to this? But 2dly, You and others have very unnecessary and aggravated fears about the dangers of ether and chloroform, and in the course of experience you will find these fears to be, in a great measure, perfectly ideal and imaginary. But the same fears have, in the first instance, been conjured up against almost all other innovations in medicine, and in the common luxuries of life. Revert again to our old simile regarding travelling. Cavendish, the Secretary to Cardinal Wolsey, tells us, in his life of that prelate, that when the Cardinal was banished from London to York by his master—that regal Robespierre, Henry VIII.,—*many* of the Cardinal's servants refused to go such an enormous journey—for they were (says Cavendish) "loath to abandon their native country, their parents, wives and children." The journey, which can *now* be accomplished in six hours, was considered *then* a perfect banishment. We travel now between London and Edinburgh (some four hundred miles) in twelve or fourteen hours. A century ago the stage coach took twelve or fourteen days. And in his life of Lord Loughborough, Lord John Campbell tells us that when he (the biographer) first travelled from Edinburgh to London, in the mail coach, the time was reduced to three

nights and two days ; but, he adds, this new and swift travelling from the Scotch to the English capital was wonderful, and I was gravely advised (adds Lord John) to stop a day at York, as several passengers who had gone through without stopping, had died of apoplexy from the rapidity of the motion." (Lives of the Lord Chancellors.

Be assured that many of the cases of apoplexy, &c. &c., alleged to arise from ether and chloroform, have as veritable an etiology as this apoplexy from rapid locomotion ; and that a few years hence, they will stand in the same light in which we now look back upon the apoplexy from travelling ten miles an hour. And as to the supposed great moral and physical evils and injuries arising from the use of ether and chloroform, they will by and by sound, I believe, much in the same way as the supposed great moral and physical evils and injuries arising from using hackney coaches at all were seriously described by Taylor, the water poet, two or three centuries ago, when these coaches were first introduced. In his diatribe against hackney coaches, Taylor warned his fellow-creatures to avoid them, otherwise, to quote his own words, "they would find their bodies tossed, tumbled, rumbled, and jumbled without mercy." "The coach (says he) is a close hypocrite ; for it hath a cover for knavery ; they (the passengers) are carried back to back in it like people surprised by pirates ; and moreover, it maketh men imitate sea-crabs in being drawn sideways," and altogether "it is a dangerous carriage for the commonwealth." Then he proceeds to call them "hell-carts," &c., and vents upon them a great deal of other abuse, very much of the same kind and character as that lavished against anæsthetics in our own day.

In the course of your remarks, you imply, I think, though you nowhere explicitly state, another objection to anæsthetics in midwifery, viz. :

5. *You object to anæsthesia in labour, because you do not consider that the mother encounters danger to her health or life from the endurance of the pains.*

"I have been accustomed (you observe) to look upon the sensation of pain in labour as a physiological relative of the power or force, and notwithstanding I have seen so many women in the throes of labour, I have always regarded a labour pain as a most desirable, salutary, and conservative manifestation of life-force."

If you hold, as your language appears to me to imply, that the sensation of pain, even when, as in labour, the degree of the pain is "absolutely indescribable," has no morbid or deleterious influence upon those who endure it, then I most decidedly disagree with you. On the contrary, I sincerely believe

that the human constitution is so constituted that it cannot endure pain, particularly when that pain is long in duration, or severe in degree, without being more or less affected and injured by it. I know of many medical and obstetric authors, from the time of Ambrose Paré down to the time of Travers, Gooch, Alison, Burns, &c., who have stated and explained the common and hitherto unchallenged opinion of our profession in all ages, that pain was, in itself, deleterious and destructive, causing depression of the heart, syncope, and even, when in excess, sometimes producing speedy and sudden death. But, till the late discovery in your own country, of the possibility of annulling the pains of surgical operations by the inhalation of ether, I know of no writer in medicine, in surgery or in midwifery who held that pain, when "absolutely indescribable" in degree, was a matter of no importance in regard to the life or health of the sufferer, and should not be relieved even when we had the complete power of relieving it.

If the mere pain of the labour were, as you state, a "desirable, salutary, and conservative manifestation of life-force," its long continuance, the very length of it, would insure, more certainly, the health and safety of the patient, than its shortness. Anything "salutary and conservative" to the constitution should manifestly be safe in proportion to the length, and dangerous in proportion to the shortness of the duration. But as far as regards the life and health of the mother, the pain of labour is perfectly the reverse of all this. It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labour, more subsequently died than when their pain does not exceed two hours; of those that are eight hours in labour, more subsequently died than of those that are four hours ill; of those that are twelve hours in suffering, more die than of those that were eight: and so on in a regular progression. The longer this supposed "salutary and conservative manifestation of life-force," as you term it, the greater became the mortality; so that, in the long run, the maternal mortality was fifty fold greater among the women that were above thirty-six hours ill, than among those who were only two hours in labour; one in every six of the former dying in childbed, and one only out of every three hundred and twenty of the latter.

Some time ago, I published a long series of statistics, tending to show, that out of a large collection of cases of the same operation performed with and without anæsthesia, those who were operated on under anæsthesia, and consequently without the usual suffering, recovered in a much larger proportion, than those who had the same operation performed without

anæsthesia, and whose constitutions were subjected to the endurance of the usual pains and agonies of the surgeon's knife.

The same result holds good, I believe, in Midwifery as in Surgery. Save the maternal constitution, either by natural or artificial anæsthesia, from the endurance of the pains connected with parturition, and you will enhance both the chances of her recovery and the facility of it. Among your red Indian and other uncivilized tribes, the parturient female does not suffer the same amount of pain during labour, as the female of the white race; and in consequence of this escape, they recover far more rapidly from the effects of parturition; nor are fatalities at all common among them. So easy is the convalescence among uncivilized tribes, that Strabo, Marco Polo, and other historians and travellers, tell us of whole communities in which the husband immediately went to bed for a number of days, upon the birth of a child, and the wife watched and nursed him. "They that write the history of America, (says Guillemeau,) tell of the women in that country, that, as soon as they be delivered, they presently rise up, and lay their husbands in their room, who are used and attended like women in child-bed."

Among the patients who have been delivered in Scotland, under anæsthesia, the rapidity of the stage of convalescence has, as a general rule, been increased in a degree that seems often to surprise the patient herself, as much as her escape from the labour pains themselves. Many of my obstetric brethren have remarked this circumstance to me. In fact, on awaking after delivery, the patient does not encounter and endure the usual feelings of exhaustion and fatigue. Some have declared to me, that they have felt as if they had awoke from a refreshing sleep. And when we consider the capabilities for the enduring of suffering and exertion, among the class of patients in civilized life upon whom you and I attend, perhaps the propriety for employing anæsthesia during labour may appear more evident. Unaccustomed by their mode of life to much pain and fatigue, patients in the higher ranks of life are not fitted to endure either of them with the same power or the same impunity as the uncivilized mother, or even as females in the lower and harder grades of civilized society; and hence there is the greater propriety and necessity in the physician employing all the means of his art, so as to save them, as far as possible, from their sufferings. To illustrate the point, let us revert again to our old comparison between the physiological functions of progression and parturition. Let us compare for a moment our ideas of the effects of fatigue from walking and of pain from parturition upon the female constitution; and surely the comparison is not an unfair one for your

views, as far as the severity of the effects of the two influences, physical fatigue and physical pain, are concerned, for surely the effects of pain, of absolutely indescribable pain, should be greater upon the constitution than mere muscular fatigue. Suppose then that our patients, at the end of the ninth month of pregnancy, had to walk on foot a continuous journey of one, two, three, six, or a dozen or more hours' duration, that is, of five, ten, twenty or thirty miles, or upwards, instead of passing through a continuous journey of recurring labour pains of the same duration, the pains gradually becoming stronger, and latterly becoming "absolutely indescribable, and comparable with no other pains"—what would be the result, with, say one hundred ladies of the upper classes of society? Some of them might be little or not at all affected by the journey; others, weak perhaps when they began, would suffer more or less severely from it. Not a few would be inclined sooner or later to stop, and beseech you, if you were the medical attendant upon them, to save them from further exertion and fatigue, by allowing them to be carried or coached the required distance.—In answer to their solicitations, would you console them by telling them that, after all, progression was a "conservative manifestation" of life-force, and free from danger, or would you take the other view, and give them means of travelling the required distance by carriage or rail? I am sure you would have recourse not to the former but to the latter, for you would fear and dread the effects of fatigue upon the fragile constitutions of your lady patients. And I repeat, that certainly the effects of the endurance of pain are as great, if not greater, upon the constitution, than the effects of the endurance of fatigue. But if you would allow your patients to ride the supposed journey, instead of unnecessarily forcing and compelling them to walk it on foot, equally, I think, should you allow them to escape what you term the "pangs and agonies of travail," by saving them by chloroform, or other anæsthetic agents, during their travail, from all the unnecessary endurance of these pangs and agonies.

You state "I have not yielded to several solicitations as to the exhibition [of chloroform] addressed to me by my patients in labour." If when driving out into the country, you perchance meet one of your fair patients, a few miles from Philadelphia, walking homeward, but so tired and way-worn that every five or ten minutes she stopped and groaned for fatigue, "absolutely indescribable and comparable to no other fatigue," I am sure you would consider yourself bound, on the principles of common humanity, not to withstand her "solicitations" to be driven home in your carriage, and thus relieved of her pre-

sent anxieties and suffering. And I cannot see why, if you do this (and who would not do it?) to relieve a patient from the mere effects of fatigue, you could refuse to relieve the same lady when in "the pangs and agonies of travail," from the endurance of pains which are, in your own words, absolutely "indescribable and comparable to no other pains."

"Perhaps (you observe) I am cruel in taking so dispassionate a view of the subject." Of course, it would ill become me to pass any such judgment upon you; but I feel this, that you and I, and other teachers of midwifery, are placed, in reference to this question, in a position far more fearfully responsible than ordinary medical practitioners. The ordinary obstetric practitioner has little or no power, except over the relief or the perpetuation, (according as he may choose it,) of the sufferings of his own immediate patients. But you and I, as obstetrical teachers, may, through our pupils, have the power of relieving or of continuing the sufferings of whole communities. If, perchance, you persist for some years longer in your present opinion, it will have the effect of inflicting a large amount of what I conscientiously believe and know to be altogether unnecessary agony and suffering upon thousands of our fellow-beings. If you review and alter your opinions, (which I earnestly hope you will do,) and make yourself sufficiently acquainted with the peculiarities in the mode of action and mode of exhibition of chloroform during labour, a vast proportion of human suffering may, even within the next few years, be saved by your happy instrumentality and influence.

Feeling as I do deeply the great responsibility in this respect of your situation and of mine, I trust you will kindly pardon and excuse me, if anywhere in the preceding pages I may have appeared to defend my views with too much earnestness. If I had to rewrite or revise the observations, I would perhaps have stated them more accurately, but I must send them as they are; and along with them I beg to send also the most sincere esteem and reiterated respects of,

My dear sir,

Yours, very faithfully,

J. Y. SIMPSON.

To Dr. Meigs,
Professor of Midwifery, Philadelphia.

THE END.